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ERRATUM.—Page 241, 19 l. from top, for Vichnow *read* Virchow
" Page 419, line 23 from top, for "forty," *read* "fifty."
" Vol. iv, p. 88, line 6 from top, for "spine," *read* "spire."

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[THIRD SERIES.]

Art. I.—*Brief Contributions to Zoölogy, from the Museum of Yale College.* No. XXIII.—*Results of Recent Dredging Expeditions on the Coast of New England*; by A. E. Verrill.

During the past summer Prof. S. F. Baird, U. S. Fish Commissioner, established his headquarters at Eastport, Me., for the purpose of investigating the fishes and fisheries of the Bay of Fundy and the adjacent waters. In addition to the investigations more immediately connected with the purposes of the Commission, Prof. Baird thought it very desirable that a general zoölogical survey, as complete as possible, should be made of the waters of that region.* He accordingly invited the writer, who had already devoted a large portion of six summers to dredging in those waters, to organize parties and construct the apparatus necessary for accomplishing this plan. It ought, perhaps, to be stated that all the persons engaged in these explorations were volunteers, the funds at the disposal of the Commission being no more than sufficient to pay for the necessary apparatus and materials required for the purpose. Very essential aid was also rendered by the officers of the U. S. Coast Survey by accommodating one party on the steamer Bache, while engaged in the survey of St. George's Bank, and giving them opportunities for dredging in that region; and by the Secretary of the Treasury, who allowed the U. S. revenue cutter Mosswood to take our parties to distant localities and aid in the dredging and other investigations. Much of the success in the explorations of the deeper waters is due to the interest which Capt. Hodgdon and the other officers of the Mosswood took in our investigations, and to the aid which they rendered us in many ways.

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* A similar exploration of the waters of Vineyard Sound, Buzzard's Bay and adjacent localities, during the entire summer of 1871, was conducted by the writer and Mr. S. I. Smith, with the aid of several other scientific gentlemen, under the auspices of the Fish Commission. A brief notice of the results of that exploration may be found in this Journal, vol. ii, page 351. A full and well-illustrated official report will soon be published. Like numerous other naturalists, Mr. J. Gwyn Jeffreys, of England, was a guest of the Fish Commission for about two weeks, and by invitation accompanied our parties on a few dredging excursions. It is to these excursions that he alludes in a recent article on "The Mollusca of Europe compared with those of Eastern North America," published in the Annals and Magazine of Natural History, vol. x, page 237, when he says that "he had dredged last autumn on the coast of New England in a steamer provided by the government of the United States." A more explicit statement would have prevented the disagreeable comments which have recently appeared in some of our newspapers from persons unacquainted with the facts.

According to the plans adopted these explorations had in view several distinct purposes, all more or less connected with the investigation of the fisheries. The special subjects attended to by this section of the Fish Commission party were chiefly the following:

1st. The exploration of the shores and shallow water for the purpose of making collections of all the algæ and marine animals living between tides, on every different kind of shore, including the numerous burrowing worms and crustacea, and to ascertain as much as possible concerning their habits, relative abundance, stations, etc.

2d. The extension of similar observations by means of the dredge, trawl, tangles, and other instruments, into all depths down to the deepest waters of the Bay of Fundy, and to make a systematic survey, as complete as possible, of all the smaller bays and harbors within our reach, both to obtain complete collections of the animals and plants and to ascertain the precise character of the bottom, special attention being paid to localities known to be the feeding grounds of valuable fishes, and to those animals upon which they are known to feed. It is believed that when the collections and notes made by the writer and his associates during previous years shall have been combined with those made during the past season, we shall have a tolerably thorough knowledge of the physical character and life of the bottom and shores in this region.

3d. The depth of the water and its temperature, both at the surface and bottom, was to be observed and recorded in as many localities as possible, and especially where dredging was to be done, and lists of the animals and plants from special localities or depths were to be prepared, so as to show the influence of temperature and other physical features upon animal and vegetable life. Many valuable observations of this kind were made. The temperature of the water was taken in numerous localities, both by Professor Baird and the dredging parties, by means of the Casella self-registering thermometers, made for this purpose and imported by Professor Baird for this expedition. These were tested by comparisons with a standard instrument.*

4th. The life of the surface waters was to be investigated by means of hand-nets and towing-nets, on every possible occasion, and at all hours. Towing-nets of different sizes, made of strong embroidery canvas, and attached to stout brass rings, were used with excellent results, but very many interesting things were obtained by hand-nets skillfully used. The surface collections made by Messrs. S. I. Smith and O. Harger in the edge of the Gulf Stream, off St. George's Bank, were of the greatest interest, and added a large number of new species to our fauna.

5th. The collections obtained were to be preserved by the best methods: 1st, for the purpose of making a more thorough study of them than could usually be done at the time, and for the purpose of insuring accuracy in their identification, and fullness in the special lists, for the final reports; and, 2d, in order to supply the Smithsonian Institution, Yale College Museum, and a number of other public museums, both American and foreign, with sets of the specimens collected. For this last purpose large quantities of duplicates were collected and preserved, and will be distributed at an early day. The alcoholic collection filled over 2,000 bottles and jars and several large cans.

6th. Those species of animals which cannot be preserved in good condition for study were to be examined with care and minutely described while living. The colors and appearance of the soft parts of other species were to be described in the same way, and also the eggs and young of all kinds.

The notes of this kind already made, chiefly by the writer and Mr. S. I. Smith, amount to more than 1,000 pages; but as we had done this in previous years for large numbers of the species, our attention was largely directed during the past season to special points and to the new and rare forms constantly obtained.

7th. It was regarded as of the utmost importance to secure accurate drawings of the living animals, and especially of such as greatly change their form and appearance when preserved, such as worms, naked mollusks, ascidians, polyps, etc. Accordingly, Mr. J. H. Emerton of Salem was employed during the month of August to make such drawings. During this time he labored most faithfully and diligently, and with remarkable results, for to his great skill with the pencil he adds the accurate observation and enthusiasm of a genuine naturalist. During the short time he was with us he made 164 drawings from living animals, many of them quite elaborate. All of these were compared critically with the living specimens at the time, and carefully corrected, even in the minute details, whenever necessary. These drawings will be used for the illustration of the final report, and will be of themselves an exceedingly valuable contribution to science, for they nearly all represent animals never before accurately figured, if at all. A large part of them are also of direct importance, since they represent the natural food of our common edible fishes.

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* Experience during the past summer has shown that these instruments are not always reliable, especially after they have been used several times in deep water. They should, therefore, be frequently compared with a standard instrument and with each other where several are used, and the error, if any, recorded for each instrument, from time to time, so that its readings may be corrected. Every instrument should be numbered or lettered, and with every observation recorded there should be a designation of the particular instrument used, otherwise errors of several degrees may occur.

8th. In all these investigations the relations existing between the fishes and the lower animals which serve as food for them, were constantly borne in mind, and all information bearing directly upon this subject that could be obtained was recorded. To this end large numbers of stomachs from fishes newly caught were examined, and lists of the species found in them were made. Most of those thus ascertained to be their ordinary food, were traced to their natural haunts from whence the fishes obtain them.

9th. The parasites of fishes, both external and internal, were collected and preserved for future study.

10th. Similar investigations, so far as practicable, were to be carried on at St. George's Bank, on the U. S. steamer Bache, in connection with the ordinary work of the U. S. Coast Survey, by a party of two, provided with all the apparatus and materials necessary for the purpose. This party at first consisted of Mr. S. I. Smith and Mr. Oscar Harger, assistants in the Yale College Museum, both of whom had previously had experience in such work. During the last cruise of the Bache they were relieved by Dr. A. S. Packard and Mr. Caleb Cooke, of Salem. Very important collections were made by both these parties, notwithstanding the unfavorable weather which they encountered.

As a general summary of the character of our collections, I may state that those made this year, together with those obtained by our parties in the same localities in previous years, but not yet reported upon, have added at least 350 species to those hitherto recorded from the same region, exclusive of Foraminifera. Entomostraca, and other minute forms. Many of these are undescribed species, but the majority are known from northern Europe. Of Polyps there were previously known 8 Actinoids and 4 Alcyonoids; we have added 4 Actinoids and 3 Alcyonoids; among the former a new and gigantic species of

Cerianthus,[*] *Urticina digitata* (Müll.), and *Bolocera Tuediæ* Gosse; and among the latter a *Pennatula*[†] near to or identical with *P. phosphorea*, a *Virgularia* near *V. mirabilis*, and a creeping species near *Cornularia*. Of Acalephs, Mr. A. Agassiz in his Catalogue of North American Acalephæ gives 48 species as known from this region; to this number we have been able to add 38 species. Of Echinoderms there have been 25 species recorded; to these we have added 10 species, including 4 Asterioids, 1 Echinoid, and 4 Holothurians; among the most interesting of these Echinoderms are *Schizaster fragilis* (Dub. and K.), obtained at five different localities, in 85 to 430 fathoms, both in the Bay of Fundy and at St. George's Bank; *Solaster furcifer* Dub. and K., *Astropecten arcticus* Sars, both from 150 fathoms, near St. George's Bank; *Pentacta assimilis* (D. and K.) and *Lophothuria squamata* (Müll. sp.) both from 430 fathoms. Of Mollusca, Dr. Stimpson enumerated 142 species in his work on the Invertebrata of Grand Menan; to this list we have added at least 95 species, including 30 of Bryozoa, 18 of Ascidians, 13 of Lamellibranchs, 30 of Gasteropods, 3 of Cephalopods. Among the most interesting of these is a fine species of *Octopus*,[‡] which we obtained from five different localities in the Bay of Fundy, at depths varying from 60 to 106 fathoms; *Trophon Gunneri* Lov.; *Ringicula nitida* V. (new sp.); *Pleurotomella Packardii* V. (sp. and gen. new), a reddish shell with a deep slit in the outer lip; the adult of *Scaphander puncto-striatus*, over an inch long; *Arca pectunculoides; Pecten pustulosus* V. (new sp.); *Glandula arenicola* V.; *Hornerea lichenoides; Defrancia lucernaria; Anarthropora borealis; Cellaria fistulosa*, etc.: all those last named were from deep water near St. George's Bank, but the *Trophon*, *Arca*, and *Cellaria* were also from the Bay of Fundy, off Grand Menan. Of Worms Dr. Stimpson enumerated 52 species, but we can easily add 125 species to his list. Among the more interesting of these are species of *Tomompteris*, *Sagitta*, *Chætoderma*, *Thalassema* (a small bright green species), *Priapulus*, *Hermione*, *Gattiola*, *Goniada*, *Scalibregma*, *Travisia*, *Ammotrypane*, *Maldane*, *Ammochares*, *Ampharete*, *Amphicteis*, *Melinna*, *Amage*, *Pista*, *Terebellides*, *Aph-*

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[*] *Cerianthus borealis*, sp. nov.

Body much elongated, tapering gradually to the abactinal opening, the surface smooth but more or less sulcated longitudinally. Marginal tentacles very numerous and unequal, the inner ones longest, in the largest specimens 2·25 inches long, and ·12 in diameter at base, gradually tapering, acute; the outer ones 1 inch and less in length. Oral tentacles numerous, crowded in several rows, in the largest specimens about 1 inch long, slender, acute. Color of body dark chestnut-brown, pale bluish just below the tentacles; disk pale yellowish-brown; space within the oral tentacles, around the mouth, deep brown, with lighter radiating lines; oral tentacles pale chestnut-brown; marginal ones deep salmon or light yellowish-brown, the longest barred transversely with six to eight dark reddish-brown spots, each spot partially divided along the median line into two lateral ones.

The two largest specimens, dredged in 28 fathoms, east of Grand Menan, by the writer, measured 5 inches across the disk and tentacles, but their bodies were mutilated. Entire ones of much smaller size were dredged by Dr. Packard and Mr. Cooke in 110 and 150 fathoms, soft mud, near St. George's Bank. The largest of these was 8 inches long, and like other species of the genus, inhabited a thick, tough, felt-like, muddy tube.

[†] This *Pennatula*, dredged by Dr. Packard and Mr. Cooke in 110 and 150 fathoms, near St. George's Bank, is apparently identical with the species dredged by Mr. J. F. Whiteaves at several localities in the Gulf of St. Lawrence in deep water, both in 1871 and 1872, but resembles ordinary European specimens of *P. phosphorea* more than do the larger specimens sent to me by Mr. Whiteaves. It will require more time and study than has yet been devoted to it to ascertain whether it be a distinct species. A *Virgularia* was also dredged last summer by Mr. Whiteaves, which may be identical with the one dredged by Dr. Packard in 150 fathoms.

[‡] *Octopus Bairdii*, sp. nov. (male alone observed). Body short, thick, somewhat depressed, broadly rounded posteriorly, separated from the head only by a slight constriction at the sides. Head almost as broad as the body, swollen above and around the eyes, concave in the middle above; around the eyes, and especially in front and above, there are numerous small conical, often irregular and rough tubercles; and a little removed from the upper side of each eye is a much larger, rough, irregularly conical, erectile tubercle, which has some small, more or less prominent, conical tubercles on its surface; the whole upper surface of the body,

head, and arms is also covered with minute scattered tubercles, which are usually but little prominent. Siphon large, tapering, capable of being bent in all directions, so as to be used for swimming both forward, backward, and sideways, according to its direction. Arms subequal, relatively short, stout, tapering to slender points, connected for about one third of their length by a web, which extends as a narrow membrane along their margins to near the ends. Suckers small, not crowded, alternating pretty regularly in the two rows; the arms of the first pair each have about 65 suckers; those of the fourth pair about 60. The right arm of the third pair has its terminal portion, for about a third of its entire length, modified for reproductive purposes into a large spoon-shaped organ, broadly elliptical in outline, with the sides incurved, somewhat trilobed at the end, deeply concave within, where there are 9 or 10 elevated transverse folds; at the base there is a fold bent into an acute angle, the apex directed forward, leaving a deep V-shaped sinus behind it, which is in continuation with a shallow groove formed by a thickening of the web along the side of the arm and terminating midway between it and the fourth arm; at the end, the arm terminates in a small conical tip, between the two broadly rounded lobes of the spoon-shaped organ; at the base of this organ there is a slight constriction, below which the basal portion bears about 31 suckers, like those on the other arms. The modified portion of the arm is considerably longer than the distance between the constriction at its base and the interbrachial web, and equal to one half the total length of the part which bears suckers. The corresponding arm on the left side is of the ordinary form and has about 51 suckers. Length of the largest specimen, in alcohol, exclusive of the arms, 1·75 inches; breadth of the body 1·25; between eyes ·7; length of the arms of the first pair, from mouth, 2·25; from mouth to edge of the web ·70; length of modified portion of third right arm ·70; breadth of this organ when expanded ·45.

When living the color was usually pale bluish-white, thickly specked with light orange brown and dark brown. Off Head Harbor, Campo Bello I., in 75 and 80 fathoms, shelly; off Herring Cove in 60 fathoms, muddy; off Grand Menan in 106 fathoms, gravel and sand.

I first dredged this interesting species while on the "Mosswood" in company with Professor Baird, in honor of whom I have named it. It was kept alive several days, and Mr. Emerton made some excellent drawings of it while living. It is somewhat related to *O. Grœnlandicus* Dewh., but the male of the latter has the third right arm much longer, with the modified portion relatively very much smaller and quite different in form, and with more numerous folds, and the basal part bears 41 to 43 suckers; the other arms also have more numerous suckers; the web is less extensive and the body is more elongated.

lebina, etc. Of Crustacea* Dr. Stimpson mentions 65 species, in the work referred to, and has added others in later papers; we have obtained, exclusive of Entomostraca, about 100 species not mentioned by him or other writers as found in this region, including 9 Decapods, 7 Schizopods, and a large number of interesting Amphipods. Among the most interesting are *Hippolyte polaris* and *H. Phippsii* (making 7 species in all); *Caridion Gordoni* Goes, from Eastport and the Bay of Fundy; *Sabinea septemcarinata* (Sabine) and *Stegocephalus ampulla* Bell, from St. George's Bank, etc. A large number of sponges, some of them of special interest, were also obtained.

Of the Algæ several thousand specimens were collected in the Bay of Fundy, representing about 65 species. These were collected and prepared chiefly by Professor Eaton, Mr. Prudden, Mr. Isham, and Mrs. Verrill. Some species were obtained which are new to the flora of our coast, among them *Laminaria dermatodea*, which was found in abundance near Eastport, at low water and also in 20 fathoms. Interesting observations on the range in depth of some of the algæ were also made. Growing specimens of *Delesseria sinuosa*, attached to Ascidians, were dredged in 75 and 80 fathoms, and were also found abundantly at various depths from low water down to 40 fathoms; *Ptilota serrata* occurred in 75 fathoms, and was also abundant at all depths less than 40 fathoms, up to low water mark, wherever the bottom was rocky. Other species occurred in depths of 20 to 50 fathoms.

The operations in the Bay of Fundy and in the bays near Eastport, were mostly under the immediate superintendence of the writer, whose party was located at Eastport. This party was quite variable in numbers, but usually consisted of at least eight or ten persons, part of whom were naturalists of considerable experience in such researches. There were twenty-five persons in all who took part in this work. Among those who were particularly useful and devoted to the work, Mr. S. I. Smith and Mr. Oscar Harger, of New Haven, who were with our party previous to their expedition to St. George's Bank in the Bache; Prof. J. E. Todd, of Tabor, Iowa; Mr. T. H. Prudden and Mr. George W. Hawes, assistants in the Sheffield Scientific School; Mr. J. B. Isham, of New Haven; Mr. J. K. Thacher, of New Haven; Mr. J. H. Emerton, of Salem; and Mr. G. Brown Goode, of Middletown, should be specially mentioned. Prof. D. C. Eaton, of New Haven, was with us a short time to collect the algæ, and many others who were able to remain only for a short time rendered important aid.

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* All the Crustacea have been put into the hands of Mr. S. I. Smith, and to him I am indebted for the identification of most of the species referred to in this paper. Mr. O. Harger has, however, consented to determine and describe the Isopoda.

A small party, consisting of Prof. H. E. Webster and Mr. Chas. Pond, of Union College, were located for a short time at Grand Menan, and made some very important collections there, both at low water and by dredging. The large lots of "sea-oranges" (*Lophothuria Fabricii* V.), *Pecten tenuicostatus*, *Mactra polynema* (St.), *Doris sulphurea*, and of *Lucernarians*, including four species, collected by them, were particularly fine. They also obtained collections of special value, because collected at the same localities where the lamented Dr. Stimpson dredged, in 1850, the specimens described in his well known work on the Invertebrata of Grand Menan. Prof. Webster and Mr. Pond had, earlier in the season, carried on extensive dredgings off Cape Porpoise, near the western extremity of the coast of Maine, at various depths down to 40 fathoms, in behalf of Union College, and obtained there an interesting collection.

Messrs. S. I. Smith and Oscar Harger were delegated to accompany the Bache to explore the St. George's Banks, but owing to various delays in the departure of the steamer they did not actually get off until the last of August, consequently the time for the operations was very much shortened and the weather was for the most part rough and stormy. They succeeded, however, in obtaining ten casts which proved to be of great interest. They made one successful dredging in 430 fathoms, which is the deepest yet done on the American coast, north of Florida.* From this single haul in 430 fathoms they obtained 44 species of animals, exclusive of Foraminifera. Among them were representatives of most of the classes of invertebrate marine animals. Some of them are of great interest and many of them quite new to American waters, although previously described from the European coast. The lines of soundings and dredgings run by the Bache were located by accurate observations, and will be reported upon hereafter, the soundings and temperature determinations being a part of the regular work of the officers of the Coast Survey. The deepest dredging (g) referred to above, was made on Sept. 15th, in N. lat. 41° 25′, W. long. 65° 42·3′; temperature of the air 66° F.; of the surface water 65°; of the bottom water 51° (the latter probably subject to considerable correction for error of the instruments).† Among the more interesting things obtained in this locality were the following, which are new to the U. S. coast, or at least have not been previously recorded :‡ *Urticina*

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* Several dredgings were made by Pourtalès, off Florida, in 450 to 600 fathoms.

† The character of the life at this locality was very similar to that prevailing in the deepest waters of the Bay of Fundy, where we found the temperature in 80 to 106 fathoms to be from 37·5° to 40°. Many of the species were identical from the two regions, and eminently arctic in character.

‡ Several of these were also dredged by our parties in the deepest waters of the Bay of Fundy, and subsequently by Dr. Packard and Mr. Cooke.

ritata (Müll. sp.); *Pentacta assimilis* (Dub. and Koren sp.);
'phothuria squamata V. (Müll. sp.); *Schizaster fragilis* (D. and
sp.); *Campanularia verticillata; Lafoëa fruticosa* Sars.; *Caly-
la fastigiata* Hincks (var. with long pedicels); *Sertularella
ıyi* H.; a new species of *Halecium*;* *Pecten pustulosus* V. (new
.): *Astarte* sp. (the same as "*A. sulcata, var. minor*," recorded
' Whiteaves from deep water in the Gulf of St. Lawrence,
.t perhaps an obese and dwarf variety of *A. lens*), *Rhyncho-
'us setosus* (Œrst. sp.); *Scalpellum* sp., etc. In addition to
ese there were several species known previously from deep
ıter, such as *Epizoanthus Americanus* V.; *Ophioglypha Sarsii;
yptodon obesus* V.; *Bela cancellata.* But the majority of the
ecies are well known inhabitants of the shallow waters of
ew England, some of them even reaching low-water mark.
nong these are *Urticina crassicornis; Tubularia indivisa; Eu-
ıdrium ramosum; Sertularella tricuspidata* Gray; *Euryechinus
'öbachiensis* V.; *Echinarachnius parma; Margarita obscura;
ıtica clausa; Mamma immaculata; Lunatia Grönlandica;
ptunea pygmæa; Entalis striolata; Leodice vivida* (St.); *Noth-
ı conchylega* Malm. (abundant), *Pycnogonum pelagicum* St.;
litu dentata Boeck; *Unciola irrorata* Say; *Pandalus annu-
ornis; Eupagurus Kroyeri* St., etc.

The bottom was composed of coarse gravel and sand, and
e specimens obtained indicate a rich and varied fauna, while
e presence of a large number of predacious species, with well
veloped eyes, shows that there must be plenty of light.

They also obtained many very interesting things from vari-
s localities on and near St. George's Bank, and Le Have
.nk, off Nova Scotia. On St. George's Bank, in N. lat. 41°
', W. long. 65° 50.3', in 60 fathoms (e), a large quantity of Hy-
oids were obtained. Among them were several species new
the American coast, viz., *Gonothyrea hyalina* Hincks; *Cam-
nularia Hincksii* Alder; *Coppinia arcta* Hincks; and a new
ecies of *Diphasia*,† remarkable for having the hydra cells

Halecium robustum, sp. nov. Stem stout and coarse, composed of many tubes;
nches stout, tapering, compound except at tips, pinnately or bipinnately
nched, the branchlets spreading at an angle of about 45°; yellowish white and
ıslucent, about ·5 of an inch long, divided by simple distant constrictions, the
ȝ internodes usually bearing from two to four hydroids. Hydrothecæ alternate,
;e, deep, somewhat vase-shaped, with an even, slightly everted rim, below
ch there is a slight constriction; the middle region is slightly smaller,
dually narrowed toward the base, with a simple diaphragm near the base
hin. The hydrothecæ are articulated upon slightly prominent projections from
stem, in an oblique and excentric position so as to produce a decidedly genicu-
d appearance. Most of the hydrothecæ are simple, but some have one or two
htly prominent secondary rims near the margin. Height about four inches.

Diphasia mirabilis, sp. nov. Stem stout, rather rigid, narrowed at base, pin-
ely branched, somewhat flexuous between the branches, which are alternate,
ıt, rigid, straight, constricted at base, spreading at an angle of about 45°.
lrothecæ on the main stem in two rows, nearly opposite; on the branches

arranged in six rows along the branches, instead of two opposite rows.

Many other interesting species occurred here, among them *Epizoanthus Americanus* V., *Cellaria fistulosa* Linn., *Stylifer Stimpsonii* V. (on *Euryechinus Dröbachiensis*), *Acmæa borealis*, *Amauropsis helicoides*, *Sabinea septemcarinata* (Sabine).

At another locality (d), a few miles from this, in N. lat. 41° 25′, W. long. 66° 25′, in 50 fathoms, sandy and shelly bottom, temperature at bottom 45°, surface 62°, Aug. 31st, a similar assemblage of species occurred. The Hydroids and Bryozoa were very numerous. Among the former the most abundant were *Hydrallmania falcata* Hincks, *Sertularia cupressina* Linn., *Diphasia fallax* Ag., *Sertularella polyzonias* Gray (var. *robusta*), *S. tricuspidata* Hincks, *Lafoëa dumosa* Sars, and *Campanularia verticillata;* but *Lafoëa fruticosa* Sars, *Campanularia Hincksii*, *C. volubilis* Alder, +*Halecium Beanii* Johns., *Calycella syringa* Hincks, *Cuspidella humilis* Hincks, *Eudendrium ramosum* Ehr., and *Tubularia indivisa* Linn., also occurred. Among the Bryozoa the most abundant were *Cellularia ternata* Johns., *C. ternata*, *var. duplex* Smitt, *Bugula Murrayana* Busk, *Cellepora scabra*, *var. plicata* Smitt, *C. avicularis* Hincks, and *Discopora Skenei* Smitt (*? var.*), the last three investing the stems of Hydroids in profusion; *Cellaria fistulosa* L., *Carberea Ellisii* Smitt, and *Farrella familiaris* (Gros. sp. = *F. pedicellata* Alder), also occurred. The Hydroids and Bryozoa from this and other localities were covered with immense numbers of Foraminifera.

At another locality (c). west of the last, in 28 fathoms, coarser sand, *Glandula arenicola* V. occurred in great abundance, associated with the ordinary sand-dwelling Mollusca. This species of Ascidian had been known before only from a few specimens dredged in Murray Bay, Can., by Dr. Dawson, and off Martha's Vineyard, by Mr. Prudden.

Off Cape Sable, N. lat. 43° 20′, W. long. 65° 21′, at the depth of 45 fathoms, the temperature of the bottom was found to be 35°, the surface being 56°, and the air 58°; upon Le Have Bank (h), N. lat. 42° 56′, W. long. 64° 51′, in 45 fathoms, the temperature of the bottom was found to be 36°, of the surface 61°, on Sept. 12. These were the lowest temperatures observed during the summer.* In accordance with these temperatures

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mostly in six regular rows, occupying all sides of the branches, those in the adjacent rows alternating. The hydrothecæ have large, appressed, somewhat swollen bases, but the upper portion is rapidly narrowed, prominent and curved outward; aperture strongly bilabiate, operculated. Reproductive capsules not observed.

* The lowest temperature observed in the Bay of Fundy was 37·5°, in 106 fathoms, east of Grand Menan, in the center of the bay. At the depth of 945 fathoms, in N. lat. 42° 18′, W. long. 64° 05′, the temperature, taken by the officers of the Bache were 62° at the surface (air 63°), and 37° at the bottom. At the depth of 1,029 fathoms, N. lat. 42° 14′, W. long. 63° 59′, they found the surface temperature 63° and the bottom 39°. N. lat. 41° 53′, W. long. 65° 6′, in 1,300

the animal life in this region was found to have a more arctic character than in most of the other localities examined.*

Among the arctic species found here were *Thuiaria articulata*, *Rhynchonella psittacea*, *Astarte elliptica*, *A. Banksii*, *Scalaria Grœnlandica*, *Aporrhais occidentalis*, *Trophon Gunneri*, *Bela violacea*, and many other northern shells; *Pteraster militaris*, *Lophothuria Fabricii* V., *Eupagurus Kroyeri* St., *Paramphithoe cataphracta* (Stimp.), *Tritropis aculeatus* Boeck; *Myriozoum subgracile*, *Eschara papposa* Pack. and *Escharoides rosacea* Smitt. The last four and the four first named were not found in any of our Bay of Fundy dredgings, and have not been found so far south before, to my knowledge. In 60 fathoms (i), near the last locality, N. lat. 42° 44′, W. long. 64° 36′, a similar assemblage of animals was met with, including the *Tritropis aculeatus*, but there were a number of additional ones of interest, among them *Acanthozone cuspidatus* Boeck, *Sabinea septemcarinata* (Sabine), *Hyas araneus*, *Turritella reticulata*, *Aglaophenia myriophyllum*, *Lafoëa fruticosa* Sars, and a peculiar sponge (*Polymastia*), growing in short stump-like masses, with a convex and verrucose summit, looking something like fragments of cauliflower. The temperature at this place was not ascertained, owing to the loss of the thermometer.

Near the entrance to Halifax harbor, off Chebucto Head, in 20 fathoms, on a bottom of soft mud and sand (j), some interesting species were dredged, and the fauna was found to be more arctic in character than even that of Grand Menan. Among the species found here were *Hyas araneus*, *Haliragés fulvocinctus* Boeck, *Aporrhais occidentalis*, *Turritella reticulata*, *Margarita varicosa*, *Astarte elliptica*, *Macoma proxima*, *Thracia myopsis*, *Rhynchonella psittacea*, etc. The temperature was not ascertained at this locality.

Among the several interesting collections made by Messrs. Smith and Harger, perhaps none are of more interest and novelty than those obtained by surface nets in the vicinity of St. George's bank, but evidently within the inner limits of the Gulf Stream, as situated at that particular date. These collections were mostly made during two days (Sept. 14th and 15th), and the weather was not very favorable for that kind of work, but the results were highly gratifying. The best localities were (l) in N. lat. 42° 3′, W. long. 63° 49′, where the temperature

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fathoms, muddy bottom, the temperature was 39°, with the surface 57°, but in N. lat. 42° 21′, W. long. 64° 9′, the temperature of the bottom in 1,120 fathoms was found to be 44°, the surface and air being 62°. It is greatly to be regretted that no dredgings could be made in any of these deeper waters.

* It is somewhat remarkable that *Crepidula unguiformis* Lam. (*plana* Say), a southern species, occurred both here and on St. George's Bank, although unknown in the Bay of Fundy. This is a species perfectly distinct from *C. fornicata*, with which some writers have united it. Frequently it occurs associated with the latter on the *outside* of shells, *Limuli*, etc.

of the surface was 72°; and (n) in N. lat. 41° 25′, W. long. 65° to 65° 25′, where the temperature varied at different hours from 66° to 70°.

Among the species obtained here were *Neptunus Sayi* Stimp., *Nautilograpsus minutus* Edw., *Latreutes ensiferus* Stimp., *Lucifer*, *Mysis*, *Thysanopoda*, *Calliopeus leviusculus*, *Hyperia*, two species, and seven other genera allied to *Hyperia*, all of which are new to the North American coast, viz: *Anchylomera*, *Oxycephalus*, *Platyscelus*, *Thyropus*, *Phrosina*, *Phronima*, *Pronoe;* besides these there were species of *Sapphirina*, *Lepas*, and many other Crustacea. Among the Acalephs were *Stomolophus meleagris* Ag., *Pelagia cyanella* Per. and Les., and *Cestum Veneris* (?) Les., of which the first two have been known before only from far south, on the coasts of Florida and S. Carolina, and the last from the Mediterranean; *Physalia Arethusa* Til., and *Charybdea periphylla* Per. and Les., were also obtained, both of which are properly Gulf Stream species. The latter has apparently been unobserved since it was briefly described in 1809, and very imperfectly figured. Of Mollusca there were *Salpæ* in abundance, three species of *Heteropods* new to our coast, and ten species of *Pteropods*, previously unknown in our waters. Among the latter are *Styliola acus* (Eschscholtz sp.), and four other species of the same genus, two of *Pleuropus*, with species of *Spiralis*, etc. Besides these, *Sagitta* was abundant, also various larval Crustacea, and attached to floating masses of *Fucus vesiculosus* and *Sargassum bacciferum* were species of Hydroids, etc. A small fish (*Motella*) was also taken at the surface.

Dr. A. S. Packard, of Salem, Mass., kindly consented to take charge of the dredging on the last cruise of the Bache, for Messrs. Smith and Harger were obliged to return to New Haven. It ought to be mentioned here, however, that he had long desired to explore the region of St. George's Bank, and had he returned from Europe earlier in the season he would doubtless have gone on the previous cruises. He was assisted by Mr. Caleb Cooke, of Salem, who has had considerable experience in collecting, both on our coast and that of Africa. They sailed on the 12th of October, and although they had but one day favorable for dredging, seven successful hauls were made in depths of 110 (o), 85 (p), 45 (q), 40 (r), and 150 fathoms (s). The first locality was somewhat to the west and north of the eastern end of the bank, in N. lat. 42° 5′, W. long. 67° 49′; the others were made in sequence, going eastward, as the steamer approached and passed over the crest of the bank, near its eastern end; but the last, in 150 fathoms, was a little farther north as well as east from the preceding ones. It will, therefore, be apparent that the region examined on this cruise was quite distant from those explored previously, and in the three deeper localities the bottom was of fine, soft, sandy mud, which was not the case in any of the previous dredgings. The presence of mud in these localities may, perhaps, be due to the fact that they are in the depression or valley between St. George's Bank and the coasts of Maine and Nova Scotia, and hence sheltered to some extent by the banks from the action of the powerful currents, which sweep over their outer sides and summit, and which appear to be sufficient to prevent the accumulation of fine sediments in that region, even at the depth of 430 fathoms, while in all shallower depths the bottom was wholly of gravel, sand, and shells. At the summit of the banks (St. George's Shoal and Cultivator Shoal) the water is very shallow,—so much so that the waves break there in heavy storms,—and the bottom is composed of moving sands, almost barren of life in some places.

The existence of powerful currents in this region was fully demonstrated this season by the Bache, and their velocity was ascertained in some localities. Mr. Smith also tells me that even far to the eastward of St. George's Bank, where no bottom was found at 1800 fathoms, the conflicting currents were sufficient to produce heavy "rips" which, even in calm weather, roared like a rapid river or like heavy breakers on a beach.

In accordance with the more sheltered positions and the muddy nature of the bottom, the animal life in the three deeper localities examined by Dr. Packard was quite different from that found on the outer side of the banks by Smith and Harger, and by me in the Bay of Fundy, at similar depths. The localities examined in the Bay of Fundy, which most resemble these, are two limited areas of muddy bottom, in depths ranging from 20 to 60 fathoms, to the eastward of Campo Bello I. and Grand Menan, which are doubtless more or less protected by those islands from the very powerful tidal currents of the bay, for in the middle of the bay we found only hard gravelly and pebbly bottoms, often pretty thickly covered with small boulders. The fauna of the deeper parts of the Gulf of St. Lawrence (where the bottom is also of soft mud) which we now know tolerably well, through the investigations of Mr. J. F. Whiteaves during the summers of 1871 and '72, resembles that of Dr. Packard's deepest dredgings much more closely than does any other yet examined. Many species from the two regions are identical and are also unknown from other localities on this side of the Atlantic. The fauna was essentially the same in the three muddy localities explored by Dr. Packard, and referred to above, viz.: in (o) 110, (p) 85, and (s) 150 fathoms; the temperatures of the surface (Oct. 12th), were 56°, 56°, and 52° F., respectively; and of the bottom 49°, 49°, and 52°. These bottom temperatures are doubtless to be corrected for a considerable instrumental error, for which I have no data. The life indicates a temperature not above 40°, and perhaps considerably lower.

As a full report, with complete lists of the species obtained on this and the previous cruises, will soon be published, it is only necessary to mention, at this time, some of the more interesting discoveries.

From the 150-fathom locality, N. lat. 42° 11′, W. long. 67° 17′, 92 species were obtained,* exclusive of Foraminifera.

Among the Radiata from this place, the most important were (u) *Bolocera Tuediæ* Gosse; (u, o) *Cerianthus borealis* V., sp. nov.; (o, p) *Pennatula*; + *Virgularia* (see note, page 5); (g) *Sertularella Gayi* Hincks; *S. tricuspidata* H.; *Sertularia cupressina* Linn.; *Ophioglypha Sarsii* Lym.; *Ophiacantha spinulosa* Müll. and Trosch.; +*Archaster arcticus* Sars; + *Solaster furcifer* Dub. and Koren; (t, g, o, p) *Schizaster fragilis* (D. and K.); *Echinarachnius parma*; +*Thyone fusus?* Koren. Of these the *Virgularia*, *Bolocera*, *Cerianthus*, *Astropecten*, *Solaster*, and *Thyone* have all been added to the American fauna this year, the last three being as yet known on this side of the Atlantic only from Dr. Packard's dredgings.

Of Mollusca there were 32 species: among the Bryozoa were +*Discoporella verrucaria* Smitt; +*Anarthropora borealis* Smitt; +*Hornerea lichenoides* Smitt; *Cellularia ternata* (*var.*) Johnst.; *Bugula Murrayana* Busk: among the Lamellibranchs were (g) *Pecten pustulosus* V., sp. nov.;† (v) *Arca pectunculoides* Scacchi; +*Neæra arctica* Sars; *Yoldia obesa* (Stimp.); *Leda tenuisulcata;*

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* In this and the following lists, the letters, o, p, q, etc., indicate that the species so marked, were also found at the localities designated by those letters (see pages 8—10, 12, etc.). The localities following s, are in the Bay of Fundy,—t is in the center of the bay, east of Grand Menan, in 95 to 106 fathoms; u is east of Grand Menan in 28 to 52 fathoms; v is west of the southern portion of Grand Menan in 40 to 60 fathoms; w, is between Campo Bello and the Wolves, in 60 fathoms; x is off Head Harbor, in 70 to 90 fathoms. The sign + indicates that the species has not been found as yet in any other locality on the coast of the United States. Species without any designation have been found in numerous localities.

† *Pecten pustulosus* V., sp. nov.

Upper valve more convex than the lower, a little swollen toward the umbo; length and breadth nearly equal, the margin diverging nearly at right angles from the beak to the middle of the anterior and posterior borders, on each of which there is an obtuse angle, from which the outline of the ventral margin forms a regular curve, nearly semicircular, but a little produced ventrally; the surface with about 14 radiating rows of relatively large, prominent, round, hollow vesicles, those in the middle rows nearly hemispherical, while part of those of the lateral ones are subconical and smaller; seven or eight of the rows are first developed, at a short distance from the apex of the shell, the other ones afterward coming in between the primary ones; the rows are distant in the middle and more crowded together toward the borders; between the rows of vesicles the surface is marked by distant, fine, impressed grooves, which pass between and separate the vesicles; on the umbos, above the origin of the vesicles, the border of the grooves rises into a thin, slightly elevated lamella. Lower valve with fine, close, slightly raised, concentric lamellæ, becoming faint toward the beak. Auricles unequal, that of the (g, i) *Astarte lens* (dwarf var.); *A. quadrans* Gould: of the Gasteropods some of the more important were +(o) *Pleurotomella Packardii* V.,* a new genus and species, allied in some respects to *Pleurotoma*, but in lacking an operculum related to *Defrancia; Admete viridula; Neptunea pygmæa* Ad.; *Aporrhais occidentalis* Sowerby; *Natica clausa* Brod. and Sowerby; *Lunatia Grönlandica;* +*Torellia vestita* Jeffreys;† *Velutina zonata* Gould; *Margarita obscura; M. cinerea* Couth.; *Scaphander puncto-striatus* H. & A. Ad. (Mighels and Adams, sp. 1841, ?=*S. librarius* Lov., 1846), an adult specimen 1·15 of an inch long, ·75 broad, with a firm, rather thick shell, destitute of epidermis, but with punctate lines

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upper valve small, and a little projecting posteriorly, much larger and more prominent, with a deep, curved emargination anteriorly, its surface with concentric lamellæ and radiating rows of small conical vesicles; that of the lower valve with a deep, angular byssal notch anteriorly, its surface with concentric lamellæ and faint radiating ridges. Color yellowish-white. Length ·30 of an inch; height ·32; thickness ·10.

Near St. George's Bank (s), in 150 fathoms, mud (living); east of St. George's (g) in 430 fathoms, sand and gravel (dead, but fresh valves).

* *Pleurotomella Packardii* V., sp. and gen. nov.

Shell thin, fragile, translucent, pale flesh-colored, moderately stout, with an acute, somewhat turreted spine. Whorls nine; the apical whorls, for about two and one half turns, nearly smooth, regular, convex, chestnut-colored; below this the whorls are shouldered, strongly convex in the middle, but with a smooth concave band below the suture, corresponding to the posterior notch in the outer lip; the whorls are crossed below the sub-sutural band by about 16 strong, prominent, rounded, somewhat oblique ribs, most prominent on the middle of the whorl, but not angulated; on the last whorl these ribs become very oblique below the middle, and follow the curve of the edge of the lip, nearly fading out anteriorly; the surface between the ribs is marked by faint lines of growth and by fine, unequal, slightly raised revolving lines, which pass over the ribs without interruption. They become more evident on the lower part of the last whorl, and are very faint on the sub-sutural band, which is more decidedly marked by receding, strongly curved lines of growth. The aperture is rather broad above, elongated below, sub-oval, outer lip very thin, sharp, prominent above, separated from the preceding whorl by a wide and very deep sinus, extending back for about one fifth of the circumference of the whorl; the anterior border of the lip is incurved near the end, and obliquely truncate, forming a short, straight canal. Columella simple, nearly straight, its inner edge toward the end, sharp and obliquely excurved. No operculum. Length ·85 of an inch, breadth ·45, length of aperture ·48, breadth of same ·20. The absence of eyes and operculum, great size of the posterior sinus, and character of the apex, indicate that this shell represents a new genus, which I purpose to call *Pleurotomella*. One living specimen from (o) 110 fathoms.

† *Torellia vestita ?* Jeffreys.

This shell in form and size somewhat resembles large specimens of *Margarita helicina*, but it has a ciliated epidermis resembling that of *Velutina haliotoidea*. The spire is small and low; whorls four, the last large, well rounded, forming the bulk of the shell. Suture deep. Umbilicus small and deep, somewhat concealed by the reflected outer edge of the columella, which recedes in front and joins the outer lip at an obtuse angle, forming a broad, shallow, anterior emargination, inner border of the columella a little excavated near the body whorl, slightly swollen in the middle. Outer lip sharp, regularly rounded. Epidermis thick, greenish, with conspicuous lines of growth, finely reticulated by raised revolving lines, along which arise numerous, slender, but short, hair-like processes. Shell beneath the epidermis white, nearly smooth. Length ·30 of an inch, breadth ·40, length of aperture ·24, breadth ·18.

Only one specimen, dead and inhabited by a *Sipunculus*, was found.

as in the smaller specimens previously known; and *Ringicula nitida* V.,* a new species allied to *R. buccinea* Desh., of Europe; *Cylichna alba; Leptochiton albus; Entalis striolata* St.; and *Dentalium occidentale* St., etc.

Among the Crustacea were *Eupagurus Kroyeri* Stimp., *E. Bernhardus* St., *E. pubescens* St., *Hyas coarctatus*, *Ptilocheirus pinguis* Stimp., *Harpina fusiformis* Smith (St. sp.), *Æga polita* Stimp., *Æ. psora* Bate and West., and *Balanus porcatus.* All of these Crustacea, except the species of *Æga*, are common in the shallow waters of the Bay of Fundy.

[To be continued.]

Art. II.—*Impressions and Footprints of Aquatic Animals and Imitative Markings, on Carboniferous Rocks;* by J. W. Dawson, LL.D., F.R.S.

The footprints and other markings of aquatic invertebrate animals and of fishes are necessarily, for the most part, less distinctive and important than those of land animals, both because less characteristic in themselves, and because reproduced under similar forms in very different geological periods. The former peculiarity has caused them to be neglected as of little importance, or to be confounded with impressions of plants. With reference to the latter, I have myself shown that the impressions made by the modern King-crab faithfully represent the *Protichnites*, *Climactichnites*, and *Rusichnites* of the Primordial and Silurian, and similar comparisons have been made by Salter, Jones, Dana and others, between the tracks of modern Crustaceans and worms and some of those in the oldest rocks.

1. *Protichnites* Owen.

The footprints from the Potsdam Sandstone in Canada, for which this name was proposed by Owen, and which were by him referred to Crustaceans probably resembling *Limulus*, were own by me in 1862* to correspond precisely with those of e American Limulus (*Polyphemus occidentalis*). I proved by periment with the modern animal that the recurring series of oups of markings were produced by the toes of the large osterior thoracic feet, the irregular scratches seen in *Protichnites neatus* by the ordinary feet, and the central furrow by the tail. t was also shown that when the Limulus uses its swimming feet produces impressions of the character of those named *Climacchnites*, from the same beds which afford *Protichnites*. The rincipal difference between *Protichnites* and their modern presentatives is that the latter have two lateral furrows prouced by the sides of the carapace, which are wanting in the rmer.

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* *Ringicula nitida* V., sp. nov.

Shell small, white, smooth, broad oval, with five whorls, spire rapidly and regularly tapered, sub-acute, shorter than the aperture. Whorls very convex, regularly rounded, the sutures well impressed; a well marked, impressed, revolving line just below the suture; the surface otherwise nearly smooth, but with more or less distinct, distant, microscopic revolving lines, most distinct anteriorly. Aperture somewhat crescent-shaped. Outer lip evenly rounded, forming the segment of a circle, the border regularly thickened, receding a little posteriorly, near the suture. Callus on the body whorl narrow, nearly even, but a little swollen in the middle and slightly raised. Columella stout, recurved at the end, with two strong, very prominent, equal, spiral folds,—the anterior one projecting beyond the canal, with the end rounded. Length ·17 of an inch; breadth ·125; length of aperture ·10; breadth of same ·043.

Two living specimens from (o) 110 and (s) 150 fathoms, muddy bottom.

As Limuloid crustaceans are well known in the Carbonifous beds of Europe and America, their footprints might be pected to occur in rocks of this age, but the first I have met ith were sent to me last summer by my friend Mr. Elder, of arvard College, who found them quite abundantly in dark-lored flag-stones belonging to the Millstone Grit formation at cKay's Head in Nova Scotia (fig. 1). The animal which proiced these marks must have been of small size (about half an ch in breadth), in this agreeing with the usual size of the al-formation Limuloids; and like the ancient Protichnite-akers, it left no trace of the edges of the carapace, but a very stinct impression of a sharp pointed tail. Its posterior feet d three or possibly four sharp toes. There were besides veral pairs of sharp-pointed walking feet. On the same slabs ere are some series of marks, evidently made by the same nd of animal, which have no tail-mark, and there are tail-arks with only traces of those of the toes. It is worthy of tice that, though these tracks indicate the presence of the anials, no crusts of Carboniferous Limuloid crustaceans have yet en found in Nova Scotia. The sand in which the tracks now ferred to were made was probably too hard to permit the imming feet to make any impression. With respect to the sence of the marks of the sides of the carapace, I may obrve that the genus *Belinurus* of the Carboniferous had the les of the carapace less deep than that of the modern Limus, and this may also have been the case with the more ancient muloids of the Potsdam. See as to this a letter by Prof. Hall the Canadian Naturalist, 1862.

To Protichnites may perhaps be referred a very singular pression from Horton Bluff (fig. 2), which at first sight much sembles *P. Scoticus*, from the Primordial of Roxburghshire, ough the Carboniferous specimen is larger and more complited.† It seems to have been produced by the successive pressure of a pair of flat organs, crenated or toothed at the edges, rather than divided into separate toes. Its horizon is the Lower Carboniferous. It was collected by Prof. Hartt.

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* Canadian Nat., vol. vii.

† Siluria, 4th edition, p. 153.

The first species of *Protichnites* referred to above may be appropriately named *P. Carbonarius*, and the second *P. Acadicus*. They are, I believe, the first impressions of this kind found in the Carboniferous.

2. *Rusichnites* Dawson.

In a paper published in the Canadian Naturalist, 1864, I showed that the singular bilobate markings with transverse striæ named *Rusophycus* by Hall, and found in the Chazy of Canada and the Clinton group of New York, are really casts of burrows connected with footprints consisting of a double series of transverse markings, and that a comparison of them with the trails and burrows of Limulus justified the conclusion that they were produced by Trilobites. I proposed for these and for similar impressions of small size found in the Carboniferous, the name given above. The Carboniferous examples I supposed might have been produced by the species of *Phillipsia* found in these beds. A specimen recently obtained from Horton shows this kind of impression passing in places into a kind of Protichnites, as if the creature possessed walking feet as well as the lamellate swimming feet which it ordinarily used.

I can scarcely doubt that the *Cruziana semiplicata* of Salter, and *C. similis* of Billings from the Primordial of Newfoundland, must have been produced by crustaceans not dissimilar from those to which *Rusichnites* belongs.

To *Rusichnites* rather than to Protichnites ought perhaps to be referred certain transverse linear impressions with a broad central groove from the Lower Carboniferous of Horton, which occur at that place under different modifications, and sometimes seem to change into light scratches or touches of feet employed in swimming, or end abruptly as if the animal had suddenly risen from the bottom.

Arenicolites Salter.

This genus may be held to include cylindrical burrows of worms with or without marks of minute setæ. They occur in rocks of all ages, and are especially abundant in the Lower Carboniferous series of Half-way River, Nova Scotia, and in the Upper Coal-formation at Tatamagouche in the same province; those at the latter place showing minute scratches produced by the setæ of the worms.* With the ordinary form at Horton there occur very long and slender, thread-like forms of the same nature with those to which the name *Nemertites* has been given.

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* Journal of the Geological Society, vol. ii.

I have long been of opinion that many of the cylindrical markings which have been described as plants under the names *Palæochorda*, *Buthotrephis*, *Palæophycus*, *Arthrophycus*, &c., are burrows of this kind, but the main difficulty seemed to be to account for their branching in a radiate or palmate manner. I have recently met with specimens from the Primordial and Carboniferous which seem to explain this. They show a central hole or burrow from which the animal seems to have stretched and withdrawn its body in different directions, so as to give an appearance of branching and radiation, possibly due merely to the excursions of the same worm from the mouth of its burrow.

No distinct examples of the Primordial and Silurian worm-trails known as *Nereites*, *Myrianites* and *Crossopodia*, have yet occurred to me in the Carboniferous.

Diplichnites Dawson.

In the Journal of the Geological Society for 1861, I described a remarkable series of impressions found at the Joggins in the Coal-formation, on the surface of a sandstone holding footprints of reptiles. It consists of two rows of strongly marked depressions about one inch long and a quarter of an inch broad (fig. 3). These marks are placed close together in each row, and the rows are six inches apart, while the space between is somewhat smoothed as if by a flat body drawn over it. The general appearance is somewhat that which would be produced by a heavy-laden toy cart six inches wide, and with broad wheels, notched or cogged at the edges, if dragged over firm sand. I suggested, in the paper above mentioned, that these singular markings might have been produced by a large crustacean or by a gigantic worm, or by a serpentiform batrachian. I have since found a very perfect but smaller series on a sandstone of the Upper Coal-formation near Toney river, which in the varying distances of the impressions seems to show that they were made by prominent movable points, while the absence of any mark or smoothing between the rows shows that the body of the animal was borne above the sand. I have hence been induced to suppose that these imprints may have been produced by the pectoral or ventral fins of fishes armed with strong spines, on which the creatures may have executed a sort of walking movement when in shallow water. In my collection from the Joggins there is a spine which I have figured and described in my Acadian Geology under the name *Gyracanthus duplicatus*, which if we can suppose it to have been a pectoral or ventral spine, would produce precisely such impressions as those of the smaller series above mentioned. The impressions of the type of Diplichnites are known to me only in the Carboniferous. Særichnites of Billings, from the Anticosti group,* has some points of resemblance to it, but is essentially distinct. My species may be named *D. ænigma.*

Rabdichnites Dawson.

Under this name I would designate the straight or slightly curved marks usually striated or grooved longitudinally, and either single or in pairs, which abound on some Carboniferous beds, and also in much older formations. At Horton Bluff, in beds holding remains of fishes and numerous footprints of crustaceans and reptiles, and scratches which were probably made by the fins of fishes, these marks abound. They were evidently furrows drawn by pointed objects trailed over the mud, and reproduced in relief on the under surfaces of the beds next deposited. Some have been produced by rounded points and are semi-cylindrical. Others are the work of chisel-shaped, pointed, notched or fimbriated organs, giving a variety of more or less close subordinate grooves or striæ. In some cases they pass into or are associated with punctures or impressions made perpendicularly like those last noticed, and this is especially the case with some of the smaller varieties. The whole of these impressions are probably marks of the spines and fins of fishes, striking the bottom or trailed over it. Some of the beds at Horton Bluff are as completely striated in this way as if glaciated, only that the striæ are individually more definite and are in all directions.

It is worthy of note that these markings strikingly resemble the so-called *Eophyton* described by Torell from the Primordial of Sweden, and by Billings from that of Newfoundland; and which also occurs abundantly in the Primordial of New Brunswick. After examining a series of these markings from Sweden shown to me by Mr. Carruthers in London, and also specimens from Newfoundland and a large number *in situ* at St. John, I am convinced that they cannot be plants, but must be markings of the nature of Rabdichnites. This conclusion is based on the absence of Carbonaceous matter, the intimate union of the markings with the surface of the stone, their indefinite forms, their want of nodes or appendages, and their markings being always of such a nature as could be produced by scratches of a sharp instrument. Since, however, fishes are yet unknown in beds of this age, they may possibly be referred to the feet or spinous tails of swimming crustaceans. Salter has already suggested this origin for some scratches of somewhat different form found in the Primordial of Great Britain. He upposed them to have been the work of species of *Hymenocaia.* These marks may, however, indicate the existence of some ree-swimming animals of the Primordial seas as yet unknown o us.

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* Report on Silurian Fossils of Anticosti, 1866.

Three other suggestions merit consideration in this connection. One is that algæ and also land plants, drifting with tides or curents, often make the most remarkable and fantastic trails. A narking of this kind was observed by Mr. G. M. Dawson last ummer to be produced by a Laminaria, and in complexity it esembled the extraordinary *Ænigmichnus multiformis* of Hitchock from the Connecticut sandstones. Much more simple narkings of this kind would suffice to give species of *Eophyon.* Another is furnished by a fact stated to the author by Prof. Morse, namely, that Lingulæ, when dislodged from their burrows, trail themselves over the bottom like worms, by means of their cirri. Colonies of these creatures, so abundant in the Primordial, may, when obliged to remove, have covered the surfaces of beds of mud with vermicular markings. The third is that the Rabdichnite-markings resemble some of the grooves in Silurian rocks which have been referred to trails of Gasteropods, as for instance, those from the Clinton group, described by Hall.

As might be expected, the markings above referred to, when in relief, occur on the under sides of the beds. A few instances may, however, be found where they exist on the upper surfaces. On careful consideration of these raised impressions, I have arrived at the conclusion that they have been left by denudation of the surrounding material, just as footprints on dry snow sometimes remain in relief after the surrounding loose snow has been drifted away by the wind; the portion consolidated by pressure being better able to resist the denuding agency. Such markings in relief on the upper surfaces of beds are, however, I believe, altogether exceptional.

It seems idle to give specific names to markings of this kind. They have evidently been made by many different species of animals, but they afford no certain characters. Fig. 4 *a* to *f*, represents some of the forms most common in the Carboniferous beds.

Imitative Markings.

Rill-marks are often very beautifully developed on the Carboniferous shales and argillaceous sandstones, though not more elaborately than on the modern mud-banks of the Bay of Fundy,[*] and they occur as far back as the oldest Cambrian.[†] Some of these simulate leaves of ferns and fronds of Laminariæ, and others resemble roots, fucoids allied to Buthotrephis, or the radiating worm-burrows already referred to.

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[*] Acadian Geology, 2nd ed., p. 26.

[†] Salter, Journal of Geol. Society, vol. xii, p. 251.

Shrinkage cracks are also abundant in some of the Carboniferous beds and are sometimes accompanied with impressions of rain-drops. When finely reticulated they might be mistaken for the venation of leaves, and when complicated with little rill-marks tributary to their sides, they precisely resemble the Dictuolites of Hall from the Medina Sandstone.

An entirely different kind of shrinkage-crack is that which occurs in certain carbonized and flattened plants, and which sometimes communicates to them a marvelous resemblance to the netted under-surface of an exogenous leaf (fig. 5). Flattened stems of plants and layers of cortical matter, when carbonized, shrink in such a manner as to produce minute reticulated cracks. These become filled with mineral matter before the coaly substance has been completely consolidated. A further compression occurs, causing the coaly substance to collapse, leaving the little veins of harder mineral matter projecting. These impress their form upon the clay or shale above and below, and thus when the mass is broken open we have a carbonaceous film or thin layer covered with a network of raised lines, and corresponding minute depressed lines on the shale in contact with it. The reticulations are generally irregular, but sometimes they very closely resemble the veins of a reticulately veined leaf. One of the most curious specimens in my possession was collected by Mr. Elder in the Lower Carboniferous of Horton Bluff. The little veins which form the projecting network are in this case white calcite; but at the surface their projecting edges are blackened with a carbonaceous film.

Slicken-sided bodies, resembling the fossil fruits described by Geinitz as *Gulielmites*, and the objects believed by Fleming and Carruthers* to be casts of cavities filled with fluid, abound in the shales of the Carboniferous and Devonian. They are, no doubt, in most cases the results of the pressure and consolidation of the clay around small solid bodies, whether organic, fragmentary or concretionary. They are, in short, local slickensides precisely similar to those found so plentifully in the coal under-clays, and which, as I have elsewhere† shown, resulted from the internal giving way and slipping of the mass as the roots of Stigmaria decayed within it. Most collectors of fossil plants in the older formations must, I persume, be familiar with appearances of this kind in connection with small stems, petioles, fragments of wood, and carpolites. I have in my collection petioles of ferns and fruits of the genus *Trigonocarpum* partially

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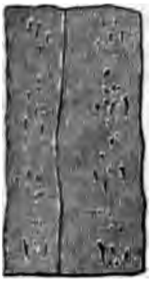
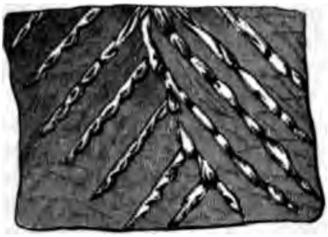
* Journal of Geol. Society, June, 1871.

† Ibid, vol. x, p. 14.

1.



2.



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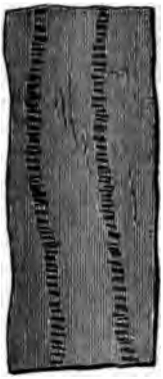
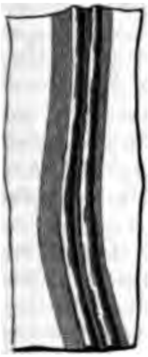
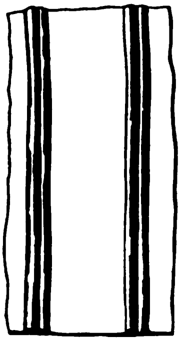


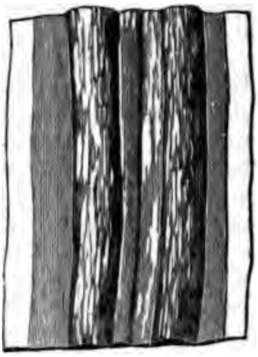
4 *a*



4 *c*

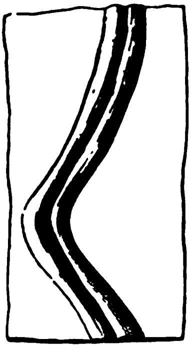


4 *d*

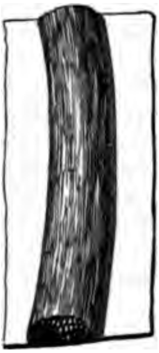


4 *b*

4 *e*



4 *f*



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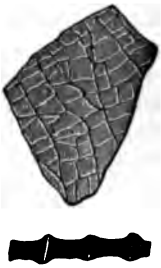


Fig. 1.—*Protichnites Carbonarius* (nat. size). Carboniferous, Nova Scotia.
Fig. 2.—*P. Acadicus*, " " "
Fig. 3.—*Diplichnites ænigma* (reduced). " "
Fig. 4.—*Rabdichnites*, different forms (nat. size). " "
Fig. 5.—Carbonized plant with reticulated markings (nat. size); *a*, enlarged section of part of the same. Carboniferous, Nova Scotia.

slicken-sided in this way, and which if wholly covered by this kind of marking could scarcely have been recognized. I have figured bodies of this kind in figs. 126 and 231 of my report on the Devonian and Upper Silurian plants, believing them, owing to their carbonaceous covering, to be probably slicken-sided fruits, though of uncertain nature. In every case I think these bodies must have had a solid nucleus of some sort, as the severe pressure implied in slicken-siding is quite incompatible with a mere "fluid-cavity," even supposing this to have existed.

Prof. Marsh has well explained another phase of the influence of hard bodies in producing partial slicken-sides, in his paper on *Stylolites*, read before the American Association in 1867, and the application of the combined forces of concretionary action and slicken-siding to the production of the cone-in-cone concretions, which occur in the Coal-formation and as low as the Primordial, was illustrated by the author in his Acadian Geology, p. 676.

Of course, as I have not seen the specimens referred by Prof. Geinitz to Gulielmites, but only the figures in his Memoir on the Permian plants of Saxony, I cannot offer any decided opinion as to their nature; but I have little doubt that the bodies mentioned by Mr. Carruthers are of the kind above referred to, and would be found to have had a solid nucleus either organic or of some other kind.

I may remark in conclusion that it would be well if collectors would give some attention to imitative markings and animal footprints of the kinds above referred to, as well as to their mode of occurrence with reference to the surfaces and material of the beds on which they are found. The labors of Hitchcock and others show how much interesting information may thus be obtained, and many mischievous errors might also be avoided. In my own studies in fossil botany, I have made it a point to collect and study all markings resembling plants, as well as the effects of crumpling, pressure, concretionary action, crystalization, shrinkage and slicken-siding upon actual vegetable remains; and by so doing I have avoided the trouble and expense of describing and figuring some dozens of imaginary species; while it would be easy to point out in works of some pretension costly figures and elaborate descriptions based on imitative forms or distorted and otherwise altered fossils.

Art. III.—*Researches in Actino-chemistry.* Memoir Second. *On the Distribution of Chemical Force in the Spectrum;* by John William Draper, M.D., LLD., President of the Faculties of Science and Medicine in the University of New York.

With scarcely an exception, the most recent works on the chemical action of radiations and spectrum analysis describe a tripartite arrangement of the spectrum, illustrated by an engraving of three curves, exhibiting the supposed relations of the calorific, the luminous, and the chemical spectra. This view, which by a mass of evidence may be shown to be erroneous, is exerting a very prejudicial effect on the progress of actino-chemistry.

I propose now to present certain facts which may aid in correcting this error. For this purpose it is necessary to show that chemical effects—decompositions and combinations—may take place in any part of the spectrum. The points to be established may be thus distinctly stated:

1st. That so far from chemical influences being restricted to the more refrangible rays, every part of the spectrum, visible and invisible, can give rise to chemical changes, or modify the molecular arrangement of bodies.

2d. That the ray effective in producing chemical or molecular changes in any special substance is determined by the absorptive property of that substance.

I may here remark that both these propositions were maintained by me many years ago; an example of the first will be found in the Philosophical Magazine (Dec., 1842), and of the second in a paper in the same journal, "On some analogies between the phenomena of the chemical rays and those of radiant heat" (Sept., 1841).

The opinion commonly held respecting the distribution of chemical force in the spectrum, is mainly founded on the behavior of some of the compounds of silver. These darken when exposed to the more refrangible rays, and unless correct methods of examination be resorted to, seem to be unaffected by the less refrangible. Hence it has been supposed, that in the higher parts of the spectrum a special principle prevails, to which the designation of "actinic rays" is often applied—an inappropriate iteration. In these pages I use the derivatives of ἀκτίς, not in this restricted sense, but as expressive of radiations of every kind. This is their proper signification.

Every part of the spectrum, no matter what its refrangibility may be, can produce chemical changes, and therefore there is no special localization of force in any limited region. Out of a large body of evidence that might be adduced, I select a few prominent instances.

1*st*—*Case of the Compounds of Silver.*

Silver is the basis of the most important photographic sensitive substances. Its iodide, bromide and chloride, darkening with rapidity under the influence of the more refrangible rays, have mainly been the cause of the misconception above alluded to respecting the tripartite constitution of the spectrum. It is necessary, therefore, to determine what are really the habitudes of these substances.

(1.) If a spectrum be received on iodide of silver, formed on the metallic tablet of the daguerreotype, and carefully screened from all access of extraneous light, both before and during the exposure, on developing with mercury vapor an impression is evolved in all the more refrangible regions. This stain corresponds in character and position to the blackening effect which under like circumstances would be found on any common sensitive silver paper. It is this which has given rise to the opinion that the so-called actinic rays exist only in the upper part of the spectrum. If, however, the action of the light be long continued, a white stain makes its appearance over all the less refrangible regions. It has a point of maximum to which I shall again presently refer.

(2.) But if the metallic tablet during its exposure to the spectrum be also receiving diffused light of little intensity, as the light of day or of a lamp, it will be found on developing that the impression obtained differs strikingly from the preceding. Every ray that the prism can transmit, from below the extreme red to beyond the extreme violet, has been active. The ultra-red heat lines α, β, γ, are present. It must be borne in mind that the impression of these lines is a proof of proper spectrum action, and distinguishes it from that of diffused light, arising either from the atmosphere or from the imperfect transparency of the prism—a valuable indication. The resulting photograph shows two well marked regions or phases of action. On its general surface, which, having condensed the mercury vapor, has the aspect of the high lights of the daguerreotype, and forms as it were the basis for the spectrum picture, there is in the region of the more refrangible rays a bluish or olive-colored impression, the counterpart of the result described in the foregoing paragraph. But in the region of the less refrangible rays no mercurial deposit has occurred, the place of those rays being depicted in metallic silver, dark, and answering to the shadows of the daguerreotype. This protected portion, which stands out in bold relief from the white background, reaches from a little below G to beyond the extreme red, and encloses the heat lines above named. They are in the form of white streaks. Though I speak of them as single lines, they are in reality groups or perhaps bands.

The general appearance of the photograph at once suggests that the less refrangible rays can arrest the action of the daylight, and protect the silver iodide from change. A close examination shows that there are three points, the extreme red, the center of the yellow, and the extreme violet, which apparently can hold the daylight in check. There are also two intervening ones in which the actions conspire. The point of maximum protection corresponds to the point of maximum action referred to above in paragraph (1).

(3.) If the metallic tablet, previously to its exposure to the spectrum, be submitted for a few moments to a weak light, so that were it developed it would at this stage whiten all over, the action of the spectrum upon it will be the same as in the last case (2). But this change in the mode of the experiment leads to a very important conclusion. The less refrangible rays can reverse or undo the change, in whatever it may consist, that light has *already* impressed on the iodide of silver.

Now bearing in mind the facts, that the photographic action of diffused light on this iodide is mainly due to the more refrangible rays it contains, we are brought by these experiments to the following conclusions:

1st. Every ray in the spectrum acts on silver iodide.

2d. The more refrangible rays apparently promote the action of the daylight on that substance; the less refrangible apparently arrest it.

3d. For the display of this arresting or antagonizing effect, it is not necessary that the less and more refrangible rays should be acting *simultaneously*. An interval may elapse and they may act *successively*. Hence the effect is not due to the contemporaneous interference of waves of different periods of vibration with one another—the material particles of the changing substance of the silver iodide are involved.

I abstain for the moment from giving further details of these spectrum impressions. That has been very completely done by Herschel, in the case of one I sent him many years ago. His examination of it, illustrated by a lithograph, may be found in the Philosophical Magazine (Feb., 1843). I shall have to return to the subject of the behavior of silver iodide in presence of radiations on a subsequent page of this memoir.

The main point at present established is this, that the silver iodide under proper treatment is affected by every ray that a flint glass prism can transmit, and therefore it is altogether erroneous to suppose that chemical force is restricted to the more refrangible portions of the spectrum.

2d.—*Case of Bitumens and Resins.*

These substances are of special interest in the history of photography, since in the hands of Niépcè they probably were the first on which impressions in the camera were obtained and fixed. Their use has been abandoned in consequence as it seems to me of an incorrect opinion of their want of sensitiveness. Properly used, they are scarcely inferior to chloride of silver.

The theory of their use is very simple. Alcohol, ether, and various volatile oils, respectively dissolve certain portions of these substances. If such a solution be spread in a thin film upon glass, as in the collodion operation, and parts of the surface be then exposed to light, the portions so exposed become insoluble in the same menstruum. They may therefore be developed by its use. Practically, care has to be taken to moderate the solvent action, and to check it at the proper time. The former is accomplished by dilution with some other appropriate liquid, the latter by the affusion of a stream of water.

The substance I have used is West India bitumen dissolved in benzine, and developed by a mixture of benzine and alcohol.

The bitumen solution being poured on a glass plate in a dark room, and drained off as in the operation of collodion, leaves a film sufficiently thin to be iridescent. This is exposed to the spectrum for five minutes, and then developed.

The beginning of the impression is below the line A, its termination beyond H. Every ray in the spectrum acts. The proof is continuous except where the Fraunhofer lines fall. A better illustration that the chemical action of the spectrum is not restricted to the higher rays, but is possessed by all, could hardly be adduced.

3d.—*Case of Carbonic Acid.*

The decomposition of carbonic acid by plants under the influence of sunshine, is undoubtedly the most important of all actino-chemical facts. The existence of the vegetable world, and indeed it may be said the existence of all living beings, depends upon it.

I first effected this decomposition in the solar spectrum, as may be found in a memoir in the Philosophical Magazine (Sept., 1843). The results obtained by me at that time from the direct spectrum experiment, that the decomposition of carbonic acid is effected by the less, not by the more refrangible rays, have been confirmed by all recent experimenters, who differ only as regards the exact position of the maximum. In the discussions that have arisen this decomposition has often incorrectly been referred to the *green* parts of plants. Plants which have been caused to germinate and grow to a certain stage in darkness are etiolated. Yet these, when brought into the sunlight, decompose carbonic acid, and *then* turn green. The chlorophyl thus produced is the effect of the decomposition, not its cause. Facts derived from the visible absorptive action of chlorophyl do not necessarily apply to the decomposition of carbonic acid. The curve of the production of chlorophyl, the curve of the destruction of chlorophyl, the curve of the visible absorption of chlorophyl, and the curve of the decomposition of carbonic acid, are not all necessarily coincident. To confound them together, as is too frequently done, is to be led to incorrect conclusions.

Two different methods may be resorted to for determining the rays which accomplish the decomposition of carbonic acid.

1st, The place of maximum evolution of oxygen gas in the spectrum may be determined. 2d, the place in which young etiolated plants turn green.

I resorted to both these methods, and obtained from them the same results. The rays which decompose carbonic acid are the same which turn etiolated plants green. They may be designated as the yellow with the orange on one side, and a portion of green on the other. Though the form of experimentation does not admit a close reference to the fixed lines, I think we are almost justified in supposing that the point of maximum action is in the yellow. It must be borne in mind, that the rapidly increasing concentration of the rays occasioned by the peculiarity of prismatic dispersion toward the red end, will give a deceptive preponderance in that direction. Without entering further into this discussion, it is sufficient for my present purpose to understand that the decomposition in question is accomplished by rays between the fixed lines B and F.

The two absorptive media, potassium bichromate, and cupro-ammonium sulphate, so often and so usefully employed in actino-chemical researches, corroborate this conclusion. Plants cannot decompose carbonic acid, nor can they turn green in rays that have passed through a solution of the latter salt. They accomplish both those results in rays that have passed through the former.

The decomposition of carbonic acid, and the production of chlorophyl by the less refrangible rays of the spectrum, afford thus a striking illustration that chemical changes may be brought about by other than the so-called chemical rays.

4th.—*Case of the Colors of Flowers.*

The production and destruction of vegetable colors by the agency of light has of course long been a matter of common observation. Little has, however, been done in the special examination of the facts, and that little for the most part by Herschel.

We have only to examine his memoir in the Philosophical Transactions (part II, 1842), to be satisfied that nearly every radiation can produce effects. Thus the yellow stain imparted by the *Corchorus Japonica* to paper is whitened by the green, blue, indigo and violet rays. The rose-red of the *Ten weeks* *stock* is in like manner changed by the yellow, orange and red. The rich blue tint of the *Viola odorata*, turned green by sodium carbonate, is bleached by the same group of rays, that is, by those less refrangible than the yellow. The green (chlorophyl) of the *Elder* leaf is changed by the extreme red.

It is needless to extend this list of examples. The foregoing establish the principle, that every part of the spectrum displays activity, some vegetable colors being affected by one, others by other rays. It is, however, desirable that the general principle at which Herschel arrived, viz: that the *luminous* rays are chiefly effective, should be more closely examined. Some important physiological explanations turn on that principle. These so-called luminous rays are such as can impress the retina, which like organic colors is a carbon compound. There are strong reasons for inferring that carbon is affected mainly by rays, the wave-lengths of which are between those of the extreme red and extreme violet, the maximum being in the yellow.

It is, however, to a former experimenter, Grotthus, that we owe the discovery of the law under which these decompositions of the colors of flowers take place. This law in repeated instances was verified by Herschel, and more recently by myself. It may be thus expressed. "The rays which are effective in the destruction of any given vegetable color are those which by their union produce a tint complementary to the color destroyed." Even the partial establishment of this law, already accomplished, is sufficient to prove that chemical effects are not limited to the more refrangible portions of the spectrum, but can be occasioned by any ray.

5th.—*Case of the union of Chlorine and Hydrogen.*

In the Philosophical Magazine (December, 1843) may be found the description of an actinometer invented by me, depending for its indications on the combination of chlorine and hydrogen, these gases having been evolved in equal volumes from hydrochloric acid by a small voltaic battery. This instrument, modified to suit their purposes, was used by Professors Bunsen and Roscoe in their photometrical researches. Many of my experiments were repeated by them (Transactions of the Royal Society, 1856, 1857).

In Table III. of my memoir, above referred to, it is shown that this mixture is affected by every ray of the spectrum; but by different ones with very different energy. The maximum is in the indigo, the action there being more than 700 times as powerful as in the extreme red.

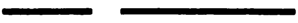
6th.—*Case of the Bending of the Stems of Plants in the Spectrum.*

It is a matter of common observation, that plants tend to grow toward the light. Dr. Gardner was, however, the first to examine the details of this phenomenon in the spectrum. His memoir is in the Philosophical Magazine (Jan., 1844). When seeds are made to germinate and grow for a few days in darkness, they develop vertical stems, very slender and some inches in length. These, on being placed so as to receive the spectrum, soon exhibit a bending motion. The stems in other parts of the spectrum turn toward the indigo; those in the indigo bend to the approaching ray. Removed into darkness, they recover their upright position. These movements are the most striking of all actinic phenomena. I have often witnessed them with admiration.

Dr. Gardner's experiments were repeated and confirmed by M. Dutrochet, who, in a report to the French Academy of Sciences (Comptes Rendus No. 26, June, 1844), added a number of facts respecting the bending of roots *from* the light, which he found to be occasioned by all the colored rays of the spectrum.

In Dr. Gardner's paper there are also some interesting facts respecting the bleaching or decolorization of chlorophyl by light. He used an ethereal solution of that substance:—

"The first action of light is perceived in the mean red rays, and it attains a maximum incomparably greater at that point than elsewhere. The next part affected is in the indigo, and accompanying it there is an action from + 10·5 to + 36·0 of the same scale (Herschel's) beginning abruptly in Fraunhofer's blue. So striking is this whole result, that some of my earlier spectra contained a perfectly neutral space, from −5·0 to +20.5, in which the chlorophyl was in no way changed, whilst the solar picture in the red was sharp and of a dazzling white. The maximum in the indigo was also bleached, producing a linear spectrum, as follows:



In which the orange, yellow and green rays are neutral. These it will be remembered are active in forming chlorophyl. Upon longer exposure the subordinate action along the yellow, etc., occurs, but not until the other portions are perfectly bleached.

"In Sir J. Herschel's experiments there remained a salmon color after the discharge of the green. This is not seen when chlorophyl is used, and is due to a coloring matter in the leaf, soluble in water, but insoluble in ether."

I have quoted these results in detail because they illustrate in a striking manner the law that *vegetable colors are destroyed by rays complementary to those that have produced them*, and furnish proof that rays of every refrangibility may be chemically active.

At this point I abstain from adding other instances showing that chemical changes are brought about in every part of the spectrum. The list of cases here presented might be indefinitely extended, if these did not suffice. But how is it possible to restrict the chemical force of the spectrum to the region of the more refrangible rays, in face of the facts that compounds of silver such as the iodide, which have heretofore been mainly relied upon to support that view and, in fact, originated it, are now proved to be affected by every ray from the invisible ultra-red to the invisible ultra-violet; how when it is proved that the decomposition of carbonic acid, by far the most general and most important of the chemical actions of light, is brought about not by the more refrangible, but by the yellow rays? The delicate colors of flowers, which vary indefinitely in their tints, originate under the influence of rays of many different refrangibilities, and are bleached or destroyed by spectrum colors complementary to their own, and, therefore, varying indefinitely in their refrangibility. Toward the indigo ray the stems of plants incline; from the red their roots turn away. There is not a wave of light that does not leave its impress on bitumens and resins, some undulations promoting their oxidation, some their deoxidation. These actions are not limited to decompositions; they extend to combinations. Every ray in the spectrum brings on the union of chlorine and hydrogen.

The conclusion to which these facts point is, then, that it is erroneous to restrict the chemical force of the spectrum to the more refrangible, or, indeed, to any special region. There is not a ray, visible or invisible, that cannot produce a special chemical effect. The diagram so generally used to illustrate the calorific, luminous, and chemical parts of the spectrum, serves only to mislead.

While thus we find that chemical action may take place throughout the entire length of the spectrum, the remarks that have been made in the previous memoir (this Journal, Sept., 1872), respecting the differences of calorific distribution in dispersion and diffraction, apply likewise to the chemical force. To be satisfied of this, it is only necessary to compare photographic impressions given by a prism and a grating!

I published engravings of such diffraction photographs in 1844. They are referred to in the Philosophical Magazine (June, 1845). As they were obtained on silver plates made sensitive by iodine, bromine and chlorine, they do not extend to the line F.

I had found that certain practical advantages arise from the use of a reflected instead of a transmitted spectrum. The ruled glass was, therefore, silvered upon its ruled face with tin amalgam, copying the surface perfectly. Of the series of spectra I used the first.

The fixed lines were beautifully represented in the photographs. They were, however, so numerous and so delicate, hat I did not attempt to do more than to mark the prominent ones. These were, I believe, the first diffraction photographs hat had ever been obtained. The wave-lengths assigned were according to Fraunhofer's scale, which represents parts of a Paris inch.

The length of the photographic impression given by the prism I was then using, from the line H to the ultra-violet end of the spectrum, was about three times that from H to G; but n the spectrum by the grating, though the exposure was in one instance continued for a whole hour, the impression beyond H was not more than 1½ times the length of that to G. In more moderate exposures the last fixed line in the photograph was about as far from H on one side as G was on the other. This, therefore, showed very clearly the difference of distribution in the diffraction and prismatic spectra.

OF THE CHEMICAL ACTION OF RADIATIONS ON SUBSTANCES.

Having offered the foregoing evidence in support of the first proposition considered in this memoir, which was to the effect—

"That so far from chemical influences being restricted to the more refrangible rays, every part of the spectrum, visible and invisible, can give rise to chemical changes, or modify the molecular arrangement of bodies," I now pass to the second, which is—

"That the ray effective in producing chemical or molecular changes in any special substance, is determined by the absorptive property of that substance."

This involves the conception of selective absorption, as I have formerly shown (Phil. Mag., Sept., 1841). A ray which produces a maximum effect on one substance may have no effect on another. Thus the rays which change chlorophyl are not those which change silver iodide.

In the examination of this subject I shall select two well known instances, presenting the fewest elements and the simplest conditions. They are, 1st, the decomposition of silver iodide, the basis of so many photographic preparations. 2d, the production of hydrochloric acid by the union of its two constituents, chlorine and hydrogen—a mixture of these gases being exceedingly sensitive to light.

1st.—*Of the decomposition of silver iodide.*

There are two forms in which the silver iodide has been used for photographic purposes: 1st, when prepared by the action of the vapor of iodine on metallic silver, as in the daguerreotype tablet; 2d, when nitrate of silver is decomposed by iodide of potassium, or other metallic iodides. These preparations differ strikingly in their actinic behavior, the former furnishing by far the most interesting series of facts.

When a polished surface of silver is exposed at common temperatures to the vapor of iodine, it speedily tarnishes, a film of silver iodide forming. This passes through several well marked tints, as the exposure continues and the thickness increases. They may be thus enumerated in the order of their occurrence: 1, lemon-yellow; 2, golden-yellow; 3, red; 4, blue; 5, lavender; 6, metallic; 7, deep yellow; 8, red; 9, green.

All these films are sensitive. Under the influence of radiations they exhibit two phases of modification. 1st, an invisible modification, which, however, can be made apparent or developed, as Daguerre discovered, by exposure to the vapor of mercury, the iodide turning white by the condensation of mercury upon it, wherever it has been exposed to light; but remaining unacted upon in parts that have been in shadow. 2d, a visible modification, which arises under a longer exposure, the iodide passing through various shades of olive and blue, and eventually becoming dark-gray.

But though all the variously tinted films of silver iodide are impressionable, they differ greatly in relative sensitiveness, when compared with each other. This may be very satisfactorily shown by producing on one silver tablet bands of all the above named colors, an effect readily accomplished by suitably unscreening successive portions of the tablet during the process of iodizing, and then exposing all at the same time to a common radiation. It will be found on developing with mercury vapor that the bands of a yellow color have been the most sensitive, those of a metallic aspect have been scarcely acted on, and those of other tints intermediately. It is to be particularly remarked that the second yellow, numbered 7 in the above series, is equally sensitive with the first yellow, numbered 2.

From this it appears that the sensitiveness of this form of iodide depends not merely on its chemical constitution, but also on its optical properties. The explanation of this different sensitiveness in different films of iodide becomes obvious when we cause a tablet prepared as just described with tinted bands to reflect the radiations falling on it to another tablet iodized to a yellow color, and placed in a camera. After due exposure and development of both with mercury, it will be found that the image of the first tablet formed on the second consists of bands of different shades of whiteness. The yellow parts of the first tablet have scarcely affected the second, but its metallic and blue parts have acted very powerfully. On comparing the first plate and its image on the second together, it will be perceived that the parts that have been affected on the one are less affected on the other.

It may therefore be inferred that the yellow films are sensiive because they absorb the incident radiation, and the metalic and blue are insensitive because they reflect it.

The effect, in whatever it may consist, which occurs during the invisible modification, is not durable: it gradually passes away. If tablets that have received impressions be kept for a ime before developing, the images upon them gradually disappear. On these tablets there is no lateral propagation of effect, nothing answering to conduction.

On examining the operation of a radiation continuously applied to one of these sensitive films, it will be discovered that a certain time elapses—that is, a certain amount of the radiation is consumed—before there is any perceptible effect. When that is accomplished the radiation affects the film to a degree proportional to its quantity, until a second stage is reached; there is then another pause followed by the second stage, in which visible modification or chemical decomposition sets in. The film begins to darken; it passes through successive tints, brown, red, olive, blue, and eventually becomes dark grey.

I have described in some of the foregoing paragraphs the action of the spectrum on silver iodide, as presented on the tablet of the daguerreotype, showing the difference in the mpression obtained: 1st, when extraneous light has been excluded; and 2d, when it has been permitted simultaneously or previously to act.

In the latter case, in all that region of the spectrum from the more refrangible extremity to somewhat below the line G, the usual darkening effect manifested by silver compounds is observed, but beyond this, and to the extreme less refrangible rays, with certain variations of intensity, the action of the extraneous and simultaneously acting light is checked, and the effect of previously acting light is destroyed.

It happened that in 1842 I obtained two very fine specimens of the latter spectra; one of these I sent to Sir J. Herschel, the other is still in my possession.

In the Philosophical Magazine (Feb., 1843), Herschel gave a detailed description of these spectrum impressions. He was disposed to refer the appearance they present to the phenomena of thin films, but at the same time pointed out the difficulties in the way of that explanation. He also sent me three proofs he had obtained on ordinary sensitive paper, darkened by exposure to light, then washed with a solution of iodide of potassium, and placed in the spectrum. He described them as follows:

(1.) “Blackened paper from which excess of nitrate of silver has not been abstracted, under the influence of an iodic salt. Produced by a November sun. N. B.—View it also transparently against the light.”

(2.) "Blackened paper under the influence of an iodic salt, when no excess of nitrate of silver exists in the paper."

(3.) "Action of spectrum under iodic influence when very little nitrate of silver remains in excess in the paper. To be viewed also transparently."

These paper photographs I still preserve. They are as perfect as when first made. The different colored spaces of the spectrum are marked upon them with pencil. The appearances they respectively present are as follows: (1) is bleached by the more refrangible rays, and blackened deeply from the yellow to the ultra-red; (2) is bleached from the ultra invisible red to the ultra-violet: a maximum occurs abruptly about the blue; (3) has the same upper spectrum as the others, a bleached dot in the center of the yellow, and a darkened space on the extreme red. The action has reached from the ultra-red to the ultra-violet.

In Herschel's opinion, these effects in the less refrangible region are connected with the drying of the paper. It is well known that paper in a damp condition is more sensitive than such as is dry. But obviously this condition does not obtain in the case of the daguerreotype operation, which is essentially a dry process.

In 1846 MM. Foucault and Fizeau, having repeated the experiment originally made by me, presented a communication to the French Academy of Sciences to the effect that when a silver tablet which has been sensitized by exposure to iodine and bromine, and then impressed by light, is exposed to the spectrum, the effect is greatly increased in all the region above the line C, and is neutralized in all that below C. They remarked the distinctness with which the atmospheric line A comes out, and saw the ultra spectrum heat-rays α, β, γ, described by me some years previously.

The interpretation given by them is, that the more refrangible rays promote the previous action of light; the less neutralize it. The curve representing the chemical intensity of the different rays would cross the axis of abscissas about the boundary of the red and orange; below that point to the ultra-red the ordinates would have negative values; above it to the ultra-violet those values would be positive (Comptes Rendus, No. 14. tome 23).

Hereupon M. Becquerel communicated to the same Academy a criticism on this interpretation, the opinion maintained by him being that while the more refrangible rays excite sensitive surfaces, the less refrangible, far from neutralizing, continue the action so begun. To the former he gave the designation "rayons excitateurs," to the latter "rayons continuateurs" (Comptes Rendus, No. 17, tome 23).

In 1847 M. Claudet communicated a paper to the Royal Society, subsequently published in the Philosophical Magazine (Feb., 1848), on this subject. His attention had been drawn to it by observing that the red image of the sun, during a dense fog, had destroyed the effect previously produced on a sensitive silver surface, and that this destruction could be occasioned at pleasure by the use of red and yellow screens. A surface which has been impressed by daylight, and the impression then obliterated by the less refrangible rays, had recovered its primitive condition. It was ready to be impressed again by daylight, and again the resulting effect might be destroyed. Claudet found that this excitation and neutralization might be repeated many times, the chemical constitution of the film remaining unchanged to the last.

These facts seem to be inconsistent with Herschel's opinion, that positive and negative pictures may succeed each other by the continued action of a radiation, on the principle of Newton's rings.

On a collodion surface such negative neutralizing or reversing actions cannot be obtained by the less refrangible rays. The spectrum impression developed in the usual manner by an iron salt presents a sudden maximum about the line G, and continues thence to the highest limit of the spectrum. In the other direction it extends below F. From E to the ultra-red not a trace of action can be detected. The lines α, β, γ, cannot be obtained on collodion. There is, therefore, a difference between its behavior under exposure to light and that of a daguerreotype tablet.

The reversals that are obtained on collodion by the use of certain haloid compounds, are altogether different from the reversals on the thin films of a silver tablet. They are produced by the *more* refrangible rays.

On exposing a collodion surface prepared in the usual manner to daylight, long enough to stain it completely, then washing off the free nitrate, and in succession dipping the plate into a weak solution of iodide of potassium, exposing it to the spectrum, washing, again dipping it in the nitrate bath, and finally developing, a reversed action is obtained. The daylight is perfectly neutralized, but not after the manner in a daguerreotype. In the region about G, the place of maximum action in collodion, the impression of the light is totally removed by an exposure of five seconds. In twelve seconds the protected space is much larger; in thirty seconds it has spread from F to H. It is, however, to be particularly remarked that the less refrangible rays show no action.

The results are substantially the same when, instead of iodide of potassium, the chloride of sodium, corrosive sublimate, bromide of potassium, or fluoride of potassium, is used. In all these the reversing action is from F to H, and has its maximum somewhere about G. That is, the reversing action coincides with the direct action; there is no protection in the lower portion of the spectrum as in the daguerreotype. The effect is altogether due to the change of composition of the sensitive film. Ordinarily it contains free nitrate; now it contains free iodide, chloride, &c.

The silver compounds of collodion absorb the radiations falling on them which are capable of producing a photographic effect. Yet sensitive as it is, collodion is very far from having its maximum sensitiveness, as is shown by the following experiment, which is of no small interest to photographers. I took five dry collodion plates, prepared by what is known as the tannin process, and having made a pile of them, caused the rays of a gas flame to pass through them all at the same time. On developing it was found that the first plate was strongly impressed, and the second, which had been behind it, apparently quite as much. Even the fifth was considerably stained. From this it follows that the collodion film, as ordinarily used, absorbs only a fractional part of the rays that can affect it. Could it be made to absorb the whole, its sensitiveness would be correspondingly increased.

Radiations that have suffered complete absorption can bring about no further change. Partial absorption, arising from inadequate thickness, may leave a ray possessed of a portion of its power. There must be a correspondence between the intensity of the incident ray and the thickness of the absorbing medium to produce a maximum effect.

Though the silver iodide is affected by radiations of every refrangibility, it is decomposed so that a subiodide results only by those of which the wave-length is less than 5000. If in presence of metallic silver, as on the daguerreotype tablet, the iodine disengaged unites with the free silver beneath. The rays of high refrangibility occasion in it chemical decomposition, those of less refrangibility, physical modification. In the language of the older theories of actino-chemistry, this substance may be said to exert a selective absorption. In this it illustrates the general principle, that it depends on the nature of the ponderable material presented to radiations which of them shall be absorbed.

(To be continued.)

ART. IV.—*On the relation between Color and Geographical Distribution in Birds, as exhibited in Melanism and Hyperchromism;* by ROBERT RIDGWAY.

(Continued from vol. iv, page 460.)

RED is a color which is similarly, or even more strongly, nodified; but in the two regions which we have mentioned it s affected in two different ways (see vol. iv, p. 454): these are 1) with *latitude*, its *intensity* increasing toward the equator; and 2) with *longitude*, its *extent* increasing toward the Pacific coast. Examples of the first case are to be found in the middle American forms of *Cardinalis Virginianus* var. *carneus** (western coast, from Colima to Realejo), and var. *coccineus*† (eastern coast, from Mirador to Honduras), in which the red is altogether deeper than in the northern forms (var. *Virginianus* and var. *igneus*).‡§ In the case of *Pyranga æstiva*, Central American specimens are quite appreciably deeper red than northern ones. In *Carpodacus frontalis*, the potency of the peculiar laws of both regions is very evident. Mexican examples (var. *hæmorrhous*)‖ are characterized by the greater intensity and more strict confinement within their normal limits of the red areas of the plumage. Pacific coast specimens (var. *rhodocolpus*)¶ show a greater or less tendency to have the red *spread*, as t were, beyond the bounds of its normal pattern. In extreme specimens from Lower California and the southern coast region of Upper California, the red even covers all the plumage, except the wings and tail; *but, in all cases, it is brightest within those limits to which it is confined in the normal pattern.* The specimens from the Middle Province of the U. S. (var. *frontalis*)[*] may be taken as representing the normal style, for it is from this central race that the two extreme differentiations diverge.

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* CARDINALIS VIRGINIANUS var. CARNEUS.
Cardinalis carneus Less., Rev. Zool., 1842, 209 (Acapulco et Relego).—Bonap. Consp., 501. (Female.)
Cardinalis Lessoni Bonap., Consp., p. 209. (Young Male.)
Hab. West Coast of middle America, from Colima to Realejo.

† CARDINALIS VIRGINIANUS var. COCCINEUS.
Cardinalis Virginianus var. *coccineus* Ridgway.
Hab. Atlantic coast of middle America, from Xalapa to Honduras; Yucatan.

‡ CARDINALIS VIRGINIANUS var. IGNEUS.
Cardinalis igneus Baird, Pr. A. N. S., 1859, 305 (Cape St. Lucas).
Hab. Cape St. Lucas, Arizona, and western Mexico, south to the Tres. Marias Islands.

§ It is somewhat remarkable that in these southern races (including var. *igneus*, in which the feature is most exaggerated) the black before the eye is much restricted, and does not cross the forehead (except sometimes very narrowly) as in the northern var. *Virginianus*.

‖ CARPODACUS FRONTALIS var. HÆMORRHOUS.
Fringilla hæmorrhoa Wagl., Isis, 1831, 525.—*Carpodacus hæm.* Scl., P. Z. S., 1856, p. 304; 1858, 303; 1859, 380.—Cat. Am. B., 1862, 122.—Baird, B. N. Am., 1858, 417.
Hab. Table-lands and elevated regions of Mexico.

¶ CARPODACUS FRONTALIS var. RHODOCOLPUS.
Carpodacus rhodocolpus Caban., Mus. Hein., p. 166.—Scl., P. Z. S., 1856, 304; 1857, 127.
Hab. Pacific coast of southern California and Mexico (south to Colima), and Peninsula of Lower California.

This tendency to an extension of red, as we approach to the Pacific coast, is strictly paralleled in the case of *Sphyropicus varius.* Taking specimens of this species from the Atlantic States (typical *S. varius*)[†], it is noticed that in the male the red patch on the throat is entirely cut off from the white rictal stripe by a continuous maxillary stripe of black, while the nuchal band is brownish white; and that the female has the throat entirely white. Not more than one per cent. have a tinge of red on the nape in the male, or a trace of it on the throat in the female. In specimens from the Rocky Mountains (var. *nuchalis*)[‡] we find that *all* have the nuchal band more or less red, while the female invariably has the throat at least one-third of this color; the male, too, has the black maxillary stripe interrupted, allowing the red of the gular patch to touch, for quite a distance, the white stripe beneath the eye, while it invades, for a greater or less extent, the black pectoral crescent. Another step is seen in specimens from the region between the Rocky Mountains and the Cascade Range, in which the red is extended still more; first, the black auricular stripe has a few touches of red, the black pectoral crescent is mixed with red feathers, and the light area surrounding it (sulphur yellow in the more eastern styles) is more or less tinged with red; then as we continue westward the red increases more and more, until in specimens from the coast region of California, Oregon, Washington Territory, and British Columbia (var. *ruber*)[§] it overspreads the whole head, neck and breast, in extreme examples entirely obliterating the normal pattern, though, usually, this can be distinctly traced. With

[*] CARPODACUS FRONTALIS, var. FRONTALIS.
Fringilla frontalis Say, Long's Exp., ii, 40.—*Carpodacus frontalis* Bonap., Consp., 533.—Baird, B. N. Am., 1858, 415.
Carpodacus familiaris McCall, P. A. N. S., vii, p. 61.
Hab. Middle Province of the U. S., including the Sierra Nevada and Rocky Mts., and southern border from the Rio Grande to Fort Tejon, Cal.

[†] SPHYROPICUS VARIUS var. VARIUS.
Picus varius Linn., S. N., i, 176.—*Sphyropicus varius* Baird, B. N. Am., 1858, 103.—Scl., Cat., 1862, 335.
Hab. Eastern Province of North America (breeding north of 40°); Mexico (both coasts).

[‡] SPHYROPICUS VARIUS var. NUCHALIS.
Sphyropicus varius var. *nuchalis* Baird, B. N. Am., 1858, 103 (sub. *S. varius*).
Hab. Rocky Mts. and middle Province of U. S.

[§] SPHYROPICUS VARIUS var. RUBER.
Picus ruber Gm., S. N., i, 1788, 429.—*Sphyropicus ruber* Baird, B. N. Am., 1858, 104.
Hab. Pacific Province of U. S. (east only to western slope of Sierra Nevada and Cascade Ranges).

is increase in the extent of red, there is also a gradually
creased amount of black, strictly parallel to that in *Picus*
llosus (var. *Harrisii*) and *P. pubescens* (var. *Gairdneri*) from the
me regions (see vol. iv, p. 456).

A case of increased intensity and prevalence of blue is clearly
lustrated in the crested jays (*Cyanura*) of the western and
terior mountain system of North America. Starting with
macrolopha, the extreme northern form of the central moun-
ins, we find the head and crest deep black, and the body
ark ashy; proceeding southward, the blue gradually increases,
rst tinging the head and crest in specimens from the southern
ocky Mountains (in which the body becomes lighter and more
luish ashy); in specimens from the central table lands of
exico (var. *diademata*) the blue tinge is much more appreci-
ble, while in those from the lower and hotter lands of Vera
ruz and Honduras (var. *coronata*) it entirely subdues the
lack, the bird being almost wholly blue.

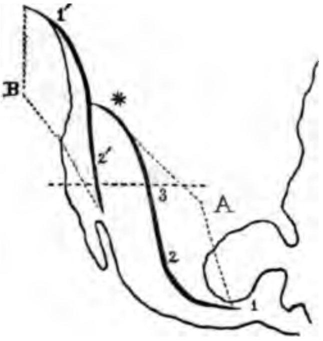
All these forms we refer to *C. coronata* (the first described),
e specific characters of which, as distinguished from the
Stelleri, in its two races, of the Pacific coast, consist in the
hite supraocular spot, barred greater wing-coverts, and dis-
nctly grayish white throat—characters shared alike by the
ree races named. It is possible, however, that even the
Stelleri may yet also have to be combined with these, for
ecimens of *C. macrolopha* from the most northern Rocky
ountains of the U. S. incline so decidedly toward *C. Stelleri*
at we either have to adopt this view, or consider them as
ybrids between the two.

C. Stelleri exists in two very well marked, though of course
tergrading, forms; the typical *stelleri* is confined to the north-
rn coast region, from Sitka to the Columbia river, and is
haracterized by a short crest, entirely black head without any
lue on the forehead, and by sooty black body, and uniform
lue of wings, belly, rump, and tail. From the Columbia
outhward, along the Sierra Nevada to southern California, it
s modified into a well marked form (var. *frontalis*), which has
longer crest, the head and crest grayish brown with con-
picuous blue streaks on the forehead, brownish ashy body,
nd two different shades of blue—the indigo of the tail and
econdaries being abruptly contrasted with the light azure of
the rump, primaries, abdomen and tail-coverts. This form
becomes more specialized, by the exaggeration of these char-
acters, as it reaches its southern limit. In the conspicuously
streaked forehead, lengthened crest, ashy body, and contrasted
shades of blue, this form approximates closely to *C. macrolopha*
of a parallel latitude, *but does not grade into it*, there never being
a white supraocular spot, barred greater coverts, or other pecu-
liar features of the Rocky Mountain and Mexican form. That

the northern race of *C. Stelleri* should grade into *C. macrolopha*, and why the southern one does not, seems to be easily explained by the following facts: The habitat longitudinally of *C. coronata* (var. *macrolopha*) within the United States is exceedingly limited, it being confined to the central ranges of the Rocky Mountain system; thus it is everywhere separated from the habitat of *Stelleri* var. *frontalis*, which is equally restricted longitudinally by that broad desert expanse, the Great Basin, which affords no sheltering woods such as are furnished on the two boundary barriers, the Sierra Nevada and the Rocky Mountains, which each represents.

The northern limit to the range of *C. macrolopha* passes just a little beyond the southern limit of the habitat of the northern race of the coast stock, and at a latitude where the Great Basin becomes greatly reduced in width, or even terminates, and where the two great mountain systems become less distinctly separated. Consequently the coast stock cannot grade into the Rocky Mountain one, by approaching its habitat, until before it becomes modified into var. *frontalis*. Thus in the parallelism of modification between *Stelleri* var. *frontalis* and *coronata* var. *macrolopha*, as we trace the two forms southward, we recognize merely the effect of a latitudinal influence. The coast stock reaches its southern limit with the Sierra Nevada, and this of course prevents it from passing into *C. coronata* var. *diademata*.

The proportionate relationship of these two forms may be more clearly illustrated by the following diagram and synopsis; the letters and figures of the former representing those of the latter:



A. Supraocular spot of white; greater wing-coverts barred with black; chin and throat abruptly lighter than adjacent parts (*C. coronata*).

1. Head and crest deep blue; back purplish blue (maximum amount of blue). *Hab.* Vera Cruz to Honduras. Var. *coronata*.*
2. Head and crest blue-black, strongly tinged with blue; back plumbeous-blue. (Intermediate form.) *Hab.* Central regions of Mexico. Var. *diademata*.†

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* CYANURA CORONATA var. CORONATA.
Cyanura coronata Swains, Phil. Mag., i, 1827, 437.
Hab. Eastern Mexico and Honduras. (North to Mirador and Xalapa.)

† CYANURA CORONATA var. DIADEMATA.
Cyanogarrulus diadematus, Bonap. Consp., 377.
Cyanura diad., Scl. Catal., 1862, 143.
Hab. Central table-lands and mountains of Mexico.

3. Head and crest deep black, scarcely tinged with blue; back ashy (minimum amount of blue). *Hab.* Rocky Mts. of the United States. Var. *macrolopha.*[*]

B. No supraocular spot of white; greater wing-coverts not barred with black; chin and throat not abruptly lighter than the adjacent portions (*C. Stelleri*).

1′. Head, crest and anterior part of the body, above and below, sooty black; no blue on the forehead; the blue of a uniform indigo shade throughout (minimum amount of blue). *Hab.* Coast Mountains of North America, from Sitka to the Columbia river. Var. *Stelleri.*[†]

2′. Head, crest and anterior part of the body, above and below, plumbeous-umber often tinged with blue, the forehead conspicuously streaked with blue; the blue of two strongly contrasted shades, indigo on secondaries and tail, and azure on tail coverts, posterior parts of the body, and primaries (maximum amount of blue). *Hab.* The Sierra Nevada, from the Columbia river to southern California. Var. *frontalis.*[‡]

Like *C. Stelleri* var. *frontalis*, all the races of *C. coronata* have the forehead streaked, but with milk-white instead of pale blue. *Stelleri* var. *frontalis*, and *coronata* var. *macrolopha*, further agree in having the longest crests, most slender bills, and most contrasted shades of blue, in the whole series; while *C. Stelleri* and *C. coronata* (as restricted) have the shortest crests, stoutest bills, and most uniform blue. These parallelisms seem to be thus directly dependent at the same time on the nearness to, and remoteness from, the central forms. In the diagram, * represents the region where the two stocks intergrade, through the mixture of the two most melanistic races; the dotted line represents the parallel of the maximum development of the crest and attenuation of the bill, and contrasted shades of blue, with excess of neither black nor blue; while "A. 1" and "B. 1′" represent the shortest crest and stoutest bill, and uniform shade of blue, with excess of blue on the one hand and of black on the other.

As a summary of these facts, it appears evident that the series of forms under consideration is divided into two well marked stocks, but that they intergrade at one point. The conclusion, then, must be that they are all modifications of one primitive species, or we must accept as the only alternative the hypothesis of hybridization. The latter theory, as an explanation of the intergrading of apparently distinct species, is fast losing favor with ornithologists who investigate deeply into the relationship of species, and may in this case, perhaps, well be abandoned.

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[*] CYANURA CORONATA var. MACROLOPHA.
Cyanocitta macrolopha Baird, P. A. N. S., 1854, 118.
Cyanura m. Baird, B. A. Am., 1858, 582.
Hab. Central Rocky Mountains of the U. S.

[†] CYANURA STELLERI var. STELLERI.
Corvus Stelleri Gm., S. N., i, 1788, 370 (Sitka).
Cyanura s. Sw., F. B. A., ii, 1831, 495. App. (descr. but not fig., which is of the intermediate form between *Stelleri* and *macrolopha !*).—Baird, B. N. Am., 1858, 581.

[‡] CYANURA STELLERI var. FRONTALIS.
Cyanura Stelleri var. *frontalis* Ridgway.

ART. V.—*On the Experimental Determination of the relative Intensities of Sounds; and on the measurement of the powers of various substances to Reflect and to Transmit Sonorous Vibrations;* by ALFRED M. MAYER, Ph.D.

(Read before the National Academy of Sciences, in Cambridge, Nov. 21, 1872.)

WHILE the problems of the determination of the pitch of sounds and the explanation of timbre have received their complete elucidation at the hands of Mersenne, Young, De la Tour, König and Helmholtz, the problem of the accurate experimental determination of the relative intensities of given sonorous vibrations has never been solved.

The method I here present will, I hope, open the way to the complete solution of this difficult and important problem; and I trust that the success I have met with will instigate others, more learned and patient, to attack with superior acumen a subject which must necessarily become of fundamental importance in the future progress of acoustic research.

1. *The determination of the relative intensities of sounds of the same pitch.*

If two sonorous impulses meet in traversing an elastic medium, and if at their place of meeting the molecules of the medium remain at rest, it is evident that at this place of quiescence the two impulses must have opposite phases of vibration, and be of equal intensity.

I have, in the following manner, experimentally applied this principle to the accurate determination of the relative intensities of vibrations giving the same note, and propagated from their sources of origin in spherical waves.

Clothe two contiguous rooms with a material which does not reflect sound, and place in each room one of the sounding bodies, and maintain these sounds of constant intensity; or the two sources of sound may be placed in the open air, and separated from each other by a non-reflecting partition. Fix at a certain distance from each sounding body a resonator responding to its note, and attach to each resonator the same length of firm gum-tubing; then lead these tubes to a forked pipe, so that the impulses from the two resonators meet at the confluence of the two branches of the forked tube, and connect the branch of the forked tube, in which the sounds meet, with one of König's manometric capsules. Now sound continuously one of the bodies, and the manometric flame when viewed in a revolving mirror will present its well known serrated appearance. On sounding the second body impulses from it will meet those from the first body, and if the phases of vibration of the impulses on the manometric membrane are opposed and of equal intensity, the membrane will remain at rest and the flame will now appear in the mirror as a band of light with a rectilinear upper border. But although the intensities of the pulses can easily be rendered equal by altering the distance of one of the resonators from its sounding body, yet this change of position will alter the relation of the phases of the impulses reaching the membrane, so that, if by mere chance, we get them opposed in the first position of the resonator, they will no longer be so after its change of position. But on stopping the vibrations of one of the bodies and setting it in vibration at intervals we may finally succeed in causing the impulses on reaching the membrane to have opposite phases of vibration. Such a method, which relies only on chance, can be of little value on account of its uncertainty and the tediousness of its application.

The above difficulty I have entirely removed by the following means. I cut a piece out of one of the tubes equal in length to a half-wave of the note we are experimenting on, and replace this piece of tubing with a glass tube of the same length, into which slides another glass tube also of half a wave in length. Now the experimentation becomes expeditious and certain. Sound both bodies continuously and place in a fixed position one of the resonators. Move the other to a certain distance from its sounding body and then pull out the inner glass tube until exact opposition of phase of the impulses is brought on to the manometric membrane. This condition will be known when the serrations have dropped to their minimum of elevation. If the latter do not entirely disappear from the band of light in the mirror, we must place the movable resonator at another distance and readjust the sliding tube. A few trials will give in the mirror a band of light with a straight, unruffled top border; then we have opposed phases of vibrations at the confluence of the branches of the forked tube, and equally intense pulses are traversing the two tubes leading from the resonators.

The distance of each resonator from its sounding body is now measured, and the inverse ratio of the squares of these distances will be the ratio of the intensities of the vibrations at the sources of the sounds, *if* the intensities of the impulses sent through a tube from a resonator varies directly with the intensities of the vibrations of the free air in the plane of the mouth of the resonator.

It will be observed that the accuracy of the determinations by this experimental method depend on three conditions. First, that the vibrating effects of the same area of a spherical sonorous wave diminish in intensity as the reciprocals of the squares of the distances of this area form the point of origin of the wave. There is every dynamic reason to believe in the truth of this proposition. The second necessary condition is that the elongation of one of the resonator tubes over the other by half a wave-length of firm glass tubing does not diminish the intensity of the impulses which have traversed it. Numerous experiments, especially those of Biot and Regnault on the aqueduct pipes of Paris, show that this short connecting tube of glass cannot in any way affect the accuracy of the measures. The third condition is that the intensities of pulses sent through a tube from a resonator vary directly with the intensities of the vibrations of the free air in the plane of the mouth of the resonator. This is a very important consideration, and as I believe there is no entirely reliable discussion of the above relation, the problem will have to be experimentally solved with the greatest care. If, however, the relations between the intensities of pulses inside the tube and those outside the mouth of the resonator shall be shown to be different (and I think they will be) from what we, for illustration, have here assumed, the process of the numerical reduction of the experiments will be only modified while the experimental method remains secure. Indeed, I cannot but consider that I have here, by applying the principle of interference, so fertile in results in optics, been the first to give an experimental method which will determine with precision the relative intensities of two sonorous vibrations producing the same note.

Savart and many other experimenters have determined the relative intensities of two sounds by placing sand or other light particles on membranes and receding from the source of sound until no motions of the particles were visible. Also Drs. Renz and Wolf (Pogg. Ann., vol. clxxiv, p. 595) give the results of experiments on the determination with the ear of the intensity of the sounds of a ticking watch. More recently Dr. Heller (Pogg. Ann., vol. ccxvii, p. 566) has made an elaborate research on the intensities of sounds; deducing mathematically his determinations from the observed amplitudes of vibration of a membrane; and Mr. Bosanquet (L. E. and D. Phil. Mag., Nov., 1872) has just published a paper in which he proposes to measure the intensities of the sounds of pipes of different pitch by the determination of the quantity of air which each pipe consumes in sounding. But all of these experimenters acknowledge the want of precision in their measures and the difficulties in the actual practice of their methods.

(To be concluded.)

ART. VI.—*On the Quartzite, Limestone and associated rocks of the vicinity of Great Barrington, Berkshire Co., Mass.;* by JAMES D. DANA. With a map.

[Continued from vol. iv, p. 453.]

2. *From the Housatonic valley westward.*

We might naturally suppose that the succession of strata in Monument mountain—gneiss, quartzite, gneiss, quartzite, in ascending order, and 800 to 1,000 feet thick (vol. iv, p. 450)—would be found to characterize the formation above the Stockbridge limestone elsewhere in Berkshire, and at least in the half a dozen miles square north of Great Barrington. But the facts now to be stated show that within a small part of this small area, the lower mica schist is in one direction replaced by quartzite; the lower quartzite, in another direction, by mica slate; and all the quartzite, mica schist and gneiss of the mountain, in another direction, by mica slate and chloritic mica slate, some of it dotted with magnetic iron. In the facts it is further shown that within a mile to the north of an east-and-west synclinal fold, there is a change from the one fold to two synclinals and an anticlinal; that several isolated north and south ranges of limestone are anticlinal emergences of a single burrowing wide-spread stratum or formation; and that some, if not all, of the high ridges of nearly vertically inclined mica slate, like that of Tom Ball, are synclinal folds of the slate.

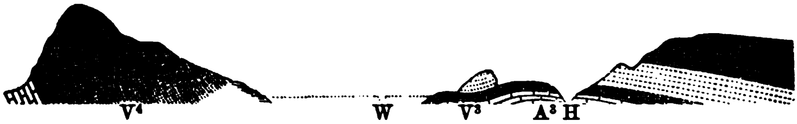
In treating of the region "from the Housatonic valley westward," I present the facts by reference to five east-and-west sections in an interval of six miles.

West of the valley in the vicinity of Housatonic village, there are the following ridges and valleys (see map, repeated, with some emendations, in this volume): 1, the quartzite ridge W, and more to the west another, lettered L; 2, the Williamsville valley, continued south in Long Pond valley; 3, the Tom Ball ridge of mica slate; 4, 5, 6, the two limestone valleys of Alford separated by a mica slate ridge (Alford Ridge).

1. A section across the Housatonic valley just below the old furnace (*f* on the map) has been given in the left part of fig. 2, on page 452 of vol. iv. Figure 3 presents the same section, and in addition its continuation westward across the Williamsville valley and Tom Ball into Alford. It shows the anticlinal of the Housatonic valley (A^3), with limestone outcropping from beneath the schist that a few hundred yards farther south covers it (as represented in section 1), and the schist overlaid by the quartzite on both sides of the river. In Williamsville valley,

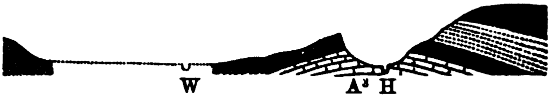
the limestone does not reappear; but instead, there is sufficient evidence that the overlying schist or slate is the upper rock under the alluvium of the river. The large masses of rock in sight are mostly boulders; yet some appear to be true outcrops, and if so indicate much variation in dip, with none at a high angle. In the slopes of Tom Ball, along this section, the slate of the lower part is nearly horizontal, or even westerly 15°, and some of it is calcareous; but forty yards above it changes to 40° to the eastward, with the strike N. 5° E.; and higher up to 50°–70°, and even 80° at the summit. The west slope of the mountain in this part is very precipitous.*

3.



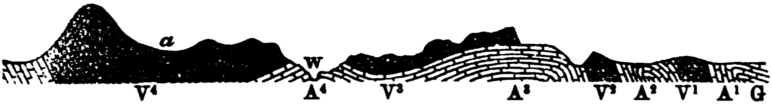
Section from Monument mountain westward, south of *f*.

4.



Section from Monument mountain westward, north of *f*.

5.



Section from Glendale westward.

2. The section in fig. 4 was taken just north of the line of the old furnace, *f* on the map, or of the cross road *f'*. The first elevation west of the Housatonic is the ridge N, which is nearly a continuation of W; but while W consists of quartzite, about N the quartzite is wanting, and there are only the underlying beds of mica slate and limestone, a bluff of mica slate forming its summit—not over 150 feet above the river. The mica slate dips to the westward 25°. West of this ridge the section is essentially the same as No. 3, no limestone outcropping in its course, and the slate being probably the upper rock underneath the alluvium.

3. *a*. Half to two-thirds of a mile farther north in the Housatonic valley, near *b'* (map), the schist on the east (right) of the

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*** In figures 3, 4, 5, W shows the position of Williams river, and H that of the Housatonic river; and the height on the west (left) is Tom Ball ridge. The letter A indicates the position of anticlinals, and V of synclinals. The parts of the three sections over one another are on the same meridian on the map. The horizontal scale is to the vertical about as one to four.**

limestone, so gently inclined near *f*, becomes nearly vertical, and the limestone adjoining has the same dip; but in the field to the westward the latter falls off in dip within 50 yards to its usual small amount, and farther west the beds disappear under the mica slate of Williamsville valley.

b. Again, half a mile farther north, near *b* on the map, the same schist, as represented at V² in fig. 5,* has the same nearly vertical position, the dip being 80° to 70° to the eastward, and in part vertical; and the limestone adjoins it with an eastward dip of 70°, strike N. 2°–5° E. To the west of the brook, along the rising road, this limestone changes its dip to 62°, and strike to N. 15°–20° W.; then, at top of the ascent, to 40°, and in a few rods to 25° to the eastward, and strike to N. 40°–60° W., the northing in the dip increasing. At the top of the ascent there is a bluff north of the road which consists of mica slate resting on the limestone, both dipping northeastward and northward; and farther westward the limestone of the here declining Housatonic fold passes out of sight under the slate.

Continuing westward along the line of the section (fig. 5) into the Williams river valley, the limestone reappears through another anticlinal: and this is the first outcrop of the limestone in this valley north of Williamsville. It comes out from beneath the mica slate, dipping in opposite directions on the opposite sides of the river, on the east eastward 15° to 25°, and on the west, or Tom Ball side, southwestward at about the same angle. The slate is very much contorted on both sides of the valley, the zigzags being often a foot in breadth. The area of emerged limestone widens rapidly to the northward, and at F Q on the map is Freedley's quarry.

Following the line of the section westward (a little south of west), up the northeastern outliers of the Tom Ball ridge (really the eastern ridge of a north-and-south synclinal), the dip of the slate continues to be southwestward at a small angle, for half a mile or more. There is then a descent to a broad depression (*a* fig. 5) partly boggy at middle, which is without outcropping rock. Beyond this rises the northwestern summit, and upon it the dip of the mica slate is 75° to 80° *to the westward*, varying to vertical. The western slope of the ridge is covered with earth; but there are outcrops of limestone at the western foot, which show that the dip on that side is 45° to 50° to the eastward, the strike N. 10–20° E.

The limestone, besides widening northward, extends around the north end of the Tom Ball ridge, and continues southward uninterruptedly through the eastern valley of Alford and beyond, nearly encircling the mountain. Tom Ball ridge is thus cut off completely from the West Stockbridge ridge, half a mile of nearly level land, with outcropping limestone, intervening between their bases.*

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* In figure 5, the part above A³ is represented too high in proportion to the height of the Tom Ball ridge to the west. The limestone A², between the two ranges of schist V¹, V², is at surface rather gently inclined for more than three fourths of its breadth.

The limestone in the northern part of the Alford valley has generally a strike of N. 10 to 20° E. (but at one place N. 8° W.), and an eastward dip of 45° to 50°; at the Churchill marble quarry (C Q, map), a north and south strike, with a dip of 90°, but a hundred yards north the dip diminishes to 40° to the eastward, and 300 south, to 30° and 35°; near Alford village, at one place the strike is N. 25° E.; at the south end of the valley N. 2° to 5° E. (but at one place N. 20° E.), the dip mostly 50 to 70°, though varying to 90°. The variations are such as occur in all the limestone regions, but less than usual, owing to the high inclination of the beds. At Churchill's marble quarry there is no division of the rock into layers through a thickness of 50 feet, while at the quarries north and south of it above referred to, where the dip is 30° to 40°, the layers are 1 to 6 or 8 feet thick. The great thickness at Churchill's quarry is owing to the soldering of many layers together, which took place at the time of metamorphism under the pressure producing the uplift and accompanying the crystallization. Such an obliteration of the layers when the dip is high is common throughout the limestone region of the Green Mountains.

Two important conclusions may be here stated.

I. The limestone of the Housatonic fold, A^3 in the sections, is the same stratum with that of the northern part of the Williamsville valley and that of Eastern Alford.

II. This limestone passes beneath the Tom Ball ridge, and the rock of this ridge is, therefore, an *overlying stratum, and has a synclinal position.*

c. The section in figure 5 extends eastward to Glendale.

The mica slate V^2, already referred to (and which extends south along by *b'* and *b''* to the western side of Monument Mountain) has a width in this section of about 200 yards. The strike is N. 2° E. to N. 5° W., and dip 60° to 80° to the eastward. Next to the eastward comes limestone, A^2; first, with a steep eastward dip (60° to 70°) like the slate; but, going eastward 50 yards, falling off to 25° and 20°. Next, at *a* on the map, there is another range of slate, V^1, with nearly the same strike (N. 5° to 10° W.), and a high dip (50° to 70°). The limestone west of it (A^2) is conformable at the junction. To the eastward there is another and more extensive range of limestone (A^1), that of

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* The latter ridge commences at D (see map), and its line is a more western one. The mica slate of the ridge is the same with that of Tom Ball, and is similarly seamed with quartz; the strike at D is N. 20°–25° E., and the dip 40°–45° E.

Glendale, which continues uninterruptedly eastward to Stockbridge; it adjoins the slate west of it (V^1) conformably; it loses its steep dip and becomes undulating over the Glendale and Stockbridge region, dipping variously but generally at a small angle to the eastward.

The slate V^1, just west of Glendale, whose position is indicated by *a* on the map, continues southward, crossing the Housatonic at the northwest bend in the river; and, with the limestones A^1 on its east side and A^2 on the west, constitutes a ridge, *a'*, which stretches southward to Monument Mountain, stopping at the elevated plain (200 feet or more above the river) of Smith's farm (*Sm* on the map). The strike of the mica slate at the bend is N. 20° W., varying, on going westward, to N. 10° E., with the dip 70° to 80° to the eastward; the western part is very rusty and decomposing.

Now near the Old Furnace (fig. 3) the limestone A^3 goes beneath the mica slate V^2, at a small angle. This limestone hence must go beneath this more steeply dipping schist, since it is a continuation of V^2, and it must emerge to the eastward either in the limestone A^2, or in A^1. But the nearly vertical dips of V^2 and V^1 (fig. 5), and the conformable yet mostly small-dipping limestone over the intermediate region, show that V^2 and V^1 are independent synclinals, and therefore that the limestone is all one and the same stratum rising in a series of anticlinals, as illustrated in the section.

Following the limestone A^3 south to Monument Mountain it appears finally to pass *beneath* the schist, though the covering of soil prevents a satisfactory examination of the junction.

In the steep slope rising above the Smith farm plateau on the south, the schist, where it outcrops, dips *westward*, which is unusual in Monument Mountain, and is apparently connected with the synclinal of mica slate that here commences. Upon this plain in the schist (near I, map) there are two very thin local beds of limestone; and one is overlaid by a local bed of quartzite a few yards thick. This is the only quartzite observed between the region of limestone about the ridge *a'* and the rocks of Monument Mountain.* The plain which is at the top of the slope (from which the steeper part of the mountain rises) is 80 or 90 feet above the Smith farm plain.

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* North of the section represented in fig. 5, in the direction of V^1, a few hundred feet north of *a* (see map), there is a steep ridge of mica slate, which is the southern outlier of the Lenox Mountain range—a line of heights between West Stockbridge and Richmond on the west and Lenox on the east, and terminating in southern Pittsfield. The dip of the slate of the ridge is mainly to the eastward at a large angle, as in Tom Ball, though 70° to the westward at this south end. The limestone of West Stockbridge in the outcrop nearest the ridge dips conformably beneath it, while 50 to 100 yards west of the slates the dip of the limestone is generally very small (25° and less) and very various as about Glendale. Lenox Mountain is probably the course of a synclinal, like the ridge *a a'* in which it seems to begin, and like Tom Ball.

The range of steeply dipping schist, V^1 (fig. 5), which near Glendale has about the same width as V^2, is not 50 feet wide (between the steeply inclined limestones A^1, A^2), at its termination just north of Smith's farm (*Sm* on map); which shows that the fold V^2 is pinched out as it nears the mountain.

From the above facts we have good grounds for the following additional conclusions.

III. The limestone of Eastern Alford, of the north end of Williamsville valley about Freedley's quarry, of the Housatonic valley, and that of Glendale and Stockbridge all belong to one stratum. Moreover, this same stratum of limestone extends from the Freedley quarry region (F Q on the map), at the north end of Tom Ball ridge, without break, I believe, *north* to West Stockbridge, three miles distant in the same Williams river valley;* and thence farther north, as I know from observation, through Richmond to Pittsfield; and also *south* from Alford into Egremont and Canaan. It is evidently one continuous mass.

IV. While in Monument Mountain there is a single broad uplift of the rocks, there are, directly north of it within a mile (section in fig. 5), two steep synclinals of mica slate; and this mica slate is part of the Monument Mountain formation.

V. In the synclinal V^2 (figure 5) the folded stratum of mica slate is a little less than 300 feet thick, since the whole breadth is 200 yards and the dip nearly vertical; and this directly overlies the limestone. Now near the Old Furnace on the Housatonic river the thickness of the mica slate over the limestone is only 50 or 60 feet; and this is overlaid in Monument Mountain by 200 or 250 feet of quartzite; and, above this, 300 feet at least of mica schist and gneiss; and then higher up another thick stratum of quartzite. Therefore, in the short distance of a mile and a half, the lower quartzite of Monument Mountain, q^1, 200 to 250 feet thick, has wholly disappeared, and instead of it there is mica slate. This quartzite, to the northward, even before reaching the line of the Old Fnrnace, is mostly well bedded, and although mainly concealed by soil; shows evidence of thinning in that direction.

VI. In the section in fig. 5, at its west end, the quartzite of Monument Mountain, q^1, overlying the schist and limestone near the Old Furnace, would naturally be looked for in the rocks of the Tom Ball ridge above the limestone. But it does not exist in any part of the slopes, nor does it outcrop to the south, except at the southern end of the ridge. Hence again the quartzite stratum has little persistence in any direction. Its representative in this region, when absent, is mica slate.

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* On the map the roads terminating at W S lead to the village of West Stockbridge, and that at C to the village of West Stockbridge Center, west of the West Stockbridge ridge.

VII. Only 300 feet of the Monument Mountain formation are folded up in each of the folds V^2 and V^1 in the section represented in fig. 5. This was probably because these folds are so small, the overlying beds having been rejected, broken up, and carried off. Two synclinals and an anticlinal are comprised within a breadth of only two-thirds of a mile.

VIII. Steep *synclinals* of slate, when the slate is associated with a thick stratum of limestone and is the *overlying* rock, should make ridges; and *per contra*, steep anticlinals if the slate is the *underlying* rock. In both cases, to produce an enduring ridge, the slate should be folded on itself so as to make a common mass; and this is not the case in an anticlinal with limestone beneath, or in a synclinal with limestone above. Hence it is that the mica slate ridges of this part of Berkshire, where nearly vertical in dip, are mainly, if not wholly, *synclinals*. The mica slate is the *overlying rock*.

IX. The mica schist and gneiss of Monument Mountain, and of regions east and south, are represented, even in the northwest part of this mountain, and in all the ridges of the region north and west of it, by the smooth mica slate (see p. 366 of vol. iv.), which is often garnetiferous, and sometimes chloritic, or dotted with magnetite. In other words, in the *same stratum* the crystalline character is most decided to the east and south; and the hydrous character (chlorite as well as the hydro-micas being hydrous minerals) most decided to the west and north.

Two more sections across this western part of the Great Barrington region remain to be described.

[To be continued.]

ART. VII.—*Observations upon the Meteors of Nov. 24th–27th*, 1872; compiled by H. A. NEWTON.

THE meteors seen upon the evenings of Nov. 24th, 25th and 27th, from their numbers, and from their probable connection with Biela's comet, are of such interest and importance as to justify a minute record of observations upon them.

November 24*th.*

1. *In New Haven.*—On the evening of Sunday, Nov. 24th, about $7^h\ 35^m$, Arthur T. Hadley, a student in Yale College, saw several meteors descend from the constellation Cygnus toward the western horizon. He called the attention of his uncle,

Prof. Twining, to the occurrence, and soon after Prof. Twining notified me. The appearance so early in November of the meteors supposed to be connected with Biela's comet had not been anticipated, and, therefore, they came upon us by surprise. Prof. Twining, previously to calling me, had made a rude approximation to the place of the radiant, considering it to be in the neighborhood of Andromeda's hand. Between half-past seven and a quarter-past nine his nephew counted 43 meteors. The actual time of watching was estimated as between 60 and 65 minutes.

The counting of the meteors was kept up with some interruptions till after midnight, with the following results.

From	8^h 15^m	to	8^h 30^m	P. M.	7	meteors, by	1	observer.
“	8 45	“	9 0	“	14	“	2	“
“	9 0	“	9 15	“	9	“	2	“
“	9 15	“	9 30	“	10	“	1	“
“	9 30	“	9 45	“	7	“	1	“
“	9 45	“	10 0	“	10	“	1	“
“	10 30	“	10 45	“	36	“	4	“
“	10 45	“	11 0	“	34	“	4	“
“	11 0	“	11 15	“	15	“	2	“
“	11 15	“	11 30	“	23	“	2	“
“	11 30	“	11 45	“	15	“	2	“
“	11 45	“	12 0	“	11	“	2	“
“	12 0	“	12 10	“	6	“	2	“
“	12 15	“	12 30	“	7	“	1	“

There was no moon, and the sky was tolerably clear. We could not command the parts within 15 or 20 degrees of the horizon because of trees and houses. Allowance in the numbers of the above table should also be made for the time I was occupied in laying down some of the tracks upon the chart.

About three fourths of the flights were conformable to a radiant region near gamma Andromedæ. Several of the unconformable ones had a peculiar faint light, and a rapid motion, giving an impression as if a white object passed rapidly by us, a few yards distant. Had they been of a dark color I might easily have imagined them to be bats, or other flying animals. These radiated from the eastern heavens, perhaps from near Orion. They were also so like each other, and so unlike the ordinary meteors, that they must belong to a group by themselves.

The following tracks, laid down upon a chart by Arthur T. Hadley and myself, will help to locate the radiant, and determine its character. The place assigned to the center of the radiant area by me at the time was two or three degrees north of γ Andromedæ.

Beginning.		Ending.		
R. A.	N. Dec.	R. A.	N. Dec.	Obs.
343	54	315	53	H.
315	35	302	29	H.
15	37	10	34½	N.
18	46	13	50	N.
34	43	41	38	N.
42	39	47	38½	N.
50	25	54	20	N.
24	23	24½	16	N.
28	24	28	17	N.
60	78½	135	81	N.
320	66	265	60	N.
345	64	335	65½	N.
358	62	342	66	N.
355	20	350	11	H.

2. *In Bethlehem, Pa.*—The number of shooting stars, as is evident from this record, would not attract general attention, being only about 40 to the hour for a single observer. Mr. R. M. Gummere, of Bethlehem, Pa., however, noticed them, and counted 40 between 10 and 11 o'clock. They appeared to him to radiate from near the zenith. Three or four were quite brilliant.

November 25th.

3. *In New Haven.*—On the morning of Monday, Nov. 25th, I watched 15^m, and saw no conformable meteors. The sky was partially clouded, and the radiant low in the N.W.

On the evening of Monday, Mr. O. Harger and the writer watched from 7^h 40^m to 9^h. The haze and clouds interfered somewhat, yet we saw 28 meteors, of which we estimated 15 to be conformable to a radiant near Andromeda. It was quite evident that a part of those seen belonged to the same group as those of the previous evening.

Later in the evening Mr. Harger, assisted by Bouton, Nevins, Tillinghast and Torrey of the Sophomore Class in Yale College, watched 3½ hours with the following results. The recorded fractions expressing the amount of sky covered by clouds are considered by Mr. Harger as too small.

Time.	Conf. meteors.	Unconf.	No. of obs.	Cloudiness.
From 10^h 25^m to 11^h 0^m	7	3	4	½ sky covered.
11 0 " 11 30	11	4	4	½ "
11 30 " 12 0	6	5	3	¾ "
12 0 " 12 30	11	24	3	⅔ "
12 30 " 1 0				⅓ "
1 0 " 1 15	4	5	3	¾ "
1 15 " 1 45				all "
1 45 " 2 0	—	—		¾ "
Total,	39 conf.	41 unconf.		

One-half of the meteors seen were, therefore, of the Andromeda group. On Sunday evening the proportion was three-fourths, implying a frequency on the earlier evening three-fold that on the later. This relative frequency is confirmed also by the absolute numbers seen.

A storm prevented further observation until Wednesday evening.

November 27th.

4. *In New Haven.*—On Wednesday evening the meteors were in such abundance as to constitute a true *star-shower*, even in Mr. Herrick's use of that word. He thought that the term should be reserved for occasions when at least 1,000 meteors per hour were visible.

A party of students upon the tower of Graduate's Hall, under my direction, began to count regularly at 6^h 38^m P. M. The periods in which fifty were counted, and the number of persons counting were as follows:

In 4^m	0^s	by 2	obs.	In 3^m	5^s	by 6	obs.	In 5^m	0^s	by 5	obs.
3	0	2	“	2	40	6	“	3	30	5	“
2	15	3	“	3	0	6	“	4	10	5	“
2	15	3	“	3	40	6	“	4	20	4	“
2	30	3½	“	3	10	6	“	3	50	4	“
1	30	4	“	3	40	6	“	5	5	4	“
2	30	4	“	2	50	6	“	6	5	4	“
2	15	6	“	3	10	6	“	6	45	4	“
2	30	6	“	15	0	6	“	3	45	4	“
2	35	6	“			6	“	9	0	4	“
3	30	6	“			6	“	7	30	4	“
2	25	6	“			6	“	Total, 1750 in $130\frac{1}{2}^m$.			

These numbers show a tolerably steady diminution in the frequency of the flights, there being 1000 in the first 56½ minutes, and 750 in the last 74 minutes. Many others, not noticed by the persons counting, were seen by Prof. Twining and myself. These were not included in their numbers. They were looking in different directions, a portion of the sky being allotted to each observer. Toward the close of the above period (8^h 48^m), the haze had increased so as to interfere seriously. There was no moon.

After twelve o'clock I watched fifteen minutes. Through breaks in the clouds enough sky was visible to assure me that the display was essentially over.

Prof. Twining was with us the latter part of the time, and made the following notes upon the characters of the meteors and the place of the radiant:

"From the 'Tower' as a station, I observed from 7^h 33^m P. M. to 8^h 45^m, giving attention to the apparent radiant, and the space on every side of it. I saw but one flight that was noticeable for length or brilliancy. This was about 12° long. It would not have been remarkable in either the displays of August 10th or of Nov. 14th. The flights were frequent, but very short, slow-moving and faint. Near the radiant they were foreshortened, as usual; but still the apparent paths were so very short, that the absolute lengths of luminous track were evidently less than those of the usual periodic meteors. Within 10° of the radiant five tracks were observed from ½° to ⅔° long, and with a duration of $0^s.3$ to $0^s.5$ of time. The luminous lines were narrow, and often *unstable*, and not in well established right lines. The longest flight in duration was about $0^s.7$, being not 5° long. Except in three instances the flights were from 1° to 4° in length, and $0^s.3$ to $0^s.6$ in duration.

"The position of the radiant was very well established. Its centre was about 43° N. Decl. and 25° R. A.; but the area of emanation seemed to be as much as 8° long. Its longer diameter was along a circle of declination and it was perhaps 3° in the cross direction."

Except to record the times and results of the counting, my own attention was confined to the determination of the place and extent of the radiant area. This was at least 8°, and was possibly 12° or more, in length, and the center of it was decided to be about 3° from γ Andromedæ in the line from the Pleiades through that star. It seemed to include the star, since in almost every direction there were flights from either side of it.* The extent was at least half as great north and south as east and west. It was difficult to determine the exact direction of the major axis of the area; I supposed it to have been nearly in the circle of declination.

The effect of the perpendicular course of the flights in shortening their real length was undoubtedly great. Those seen by me were also generally near the radiant, and thus greatly foreshortened. But the bodies, I am convinced, were really smaller, and would therefore in any case be more quickly consumed than the usual August and November meteors.

The only two trains which I placed upon the chart were—beginning, 25° R. A., 41° Dec.; end, 23½° R. A., 38° Dec. Beginning, 26° R. A., 44° Dec.; end, 26½° R. A., 48° Dec.

At the Sheffield Scientific School, Prof. Lyman and Mr. G. W. Hawes counted 64 meteors in six minutes between 5^h 55^m and 6^h 1^m, P. M.; Mr. T. M. Prudden and Mr. Hawes counted 67 in 12 minutes, between 7^h 35^m and 7^h 47^m. In 12½ minutes, between 7^h $47\frac{1}{2}^m$ and 8^h, these three gentlemen counted 81, and between 8^h 4^m and 8^h 21^m, or in 17 minutes, they saw 100. Prof. Lyman placed the center of the radiant at R. A. 25° and N. Dec. 43°. He remarked that it was most obviously scattered.

In 5 minutes near 6^h P. M., Mr. C. W. Chapman, who was at my house, counted 30 flights. Another observer, viewing part of the sky, counted 50 meteors between $7^h 35^m$ and $7^h 56^m$. On Thursday I saw no meteors in 15 minutes between $5\frac{1}{2}^h$ and 6^h, P. M. On Friday, from $8^h 53^m$ to $9^h 8^m$, P. M., I saw 3 meteors in a clear sky, neither coming from Andromeda. On Saturday, from $5^h 40^m$ to 6^h, A. M., I saw 8 flights in the eastern sky, three of which might have come from Andromeda, then in the N.W. Four were from near the zenith, perhaps from Leo.

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* Prof. Twining says that it did most certainly include the star.

5. *In Washington.*—The following report of Rear Admiral Sands to the Secretary of the Navy, is copied from the N. Y. *Herald*:—"I have the honor to report that last night, being clear, a fine display of meteors was observed by Professor Eastman and Mr. Horrigan, watchman of the observatory. In the early evening, Professor Eastman being occupied in other duties, Mr. Horrigan observed 485 meteors between $6^h 15^m$ and 8, P. M. From 8^h to 9^h, P. M., Professor Eastman observed part of the time, and 131 were seen; after 9^h, P. M., 100 more were seen, and at 10^h, P. M., the display seemed to cease. The maximum flight appears to have been between the hours of $6^h 15^m$ and $6^h 30^m$, reaching an hourly rate of 102 in 15 minutes, and of 400 in 8 minutes. Mr. Horrigan saw 10 before he commenced the above record, making the whole number observed 720. They appeared to radiate, by Professor Eastman's observation, from a space which might be enclosed by a circle of eight degrees in diameter, having its center at μ Andromedæ. Professor Harkness also observed, but differs a little as to the point of radiance, placing it about half way between μ and γ Andromedæ. As there was but a single observer in the early evening, whose attention was confined to one portion of the heavens, there were probably four and a half times as many fell as were observed. According to Professor Harkness' observations, the most of the meteors were about of the fourth magnitude. The color to the naked eye was generally faint blue, but some of the larger were reddish. The tracks were generally very short, not exceeding from four degrees to six degrees. The average time of flight was from one to two-tenths of a second. Professor Eastman succeeded in catching the spectra of two small ones. The first had a faint continuous spectrum with an excess of yellow or greenish yellow; the second had a faint green spectrum, the first glimpse of which appeared perfectly white. They were both very faint and moved rapidly. This display is a very remarkable one, and exceeds that ordinarily seen on the 14th and 15th of November. The radiant point seems to indicate that they are moving in the orbit of Biela's comet."

6. In a letter to the editors, Prof. A. Hall, of Washington, reports the following observations.

From $6^h 25^m$ to $6^h 43^m$, Washington mean time, one hundred meteors were counted, and from $7^h 40^m$ to $8^h 0^m$, fifty were counted. Most of the meteors were small, and only four or five near the zenith left trails that endured a few seconds. The sky was clear to within 10° or 15° of the horizon. By a rough estimation, from a few of the tracks traced upon a globe, the radiant was located at R. A. 355°, Dec. +43°.

From this position of the radiant, Prof. Hall computed the following parabolic orbit by the formulas of Dr. Weiss.

Elements of Meteors.	Biela's Comet.
$\pi = 89^{\circ}\cdot5$	$\pi = 109^{\circ}\cdot0$
$\Omega = 246^{\circ}\cdot1$	$\Omega = 245^{\circ}\cdot9$
$i = 15^{\circ}.4$	$i = 12^{\circ}.6$
$\log q = 9\cdot976$	$\log q = 9\cdot935$

7. Prof. S. Newcomb also writes to the editors as follows:

"This evening between 6^h 50^m and 7^h, W. M. T., meteors fell at a mean rate of perhaps 15 or 20 per minute. During the few minutes I watched, my attention was given to the region near the radiant point for the purpose of fixing the position of the latter. It was quite near the star 50 Andromedæ, probably a degree or so to the northeast, which would make its position

R. A. 1^h 32^m Dec. + $41\frac{1}{2}$°.

The uncertainty of this position I suppose to be about 2°. At 7^h 20^m the frequency had diminished so rapidly that it was no longer possible to estimate the position of the radiant with precision. This accidental opportunity to determine it may therefore have been fortunate enough to warrant the publication of its result."

8. *In Rochester, N. Y.*—Mr. Lewis Swift, in Rochester, N. Y., counted in an hour and a half, between 9^h and 11^h, P. M., 51 meteors. All but one radiated from γ Andromedæ, or more exactly from a point about one fourth the distance from γ to β. At the beginning of his observations the radiant was exactly at γ, but at the end was as stated above. They were mostly small and moved more slowly than ordinary meteors.

On the next evening (Nov. 28th) the sky was clear, but there were no meteors.

9. *In Philadelphia, Pa.*—Mr. B. V. Marsh, on Wednesday evening, about 10 minutes after 6, counted 11 meteors in a minute or two. He then kept watch for an hour or two with the following results:

From 6^h 15^m	to 6^h 30^m	50	meteors,	or 200	to the hour.
" 6 30	" 6 45	38	"	152	"
" 7 10	" 7 30	40	"	120	"
" 7 50	" 7 58	12	"	90	"
" 11 30	" 11 45	3	"	12	"
	Total,	143			

He adds: "I presume that the maximum had passed before I commenced my count, as there was a steady diminution in the hourly rate, and I am confident that the interval during which

I saw the first 11 did not exceed two minutes, giving an hourly rate of 300.

"The radiation was diffuse. The principal radiant I thought was about R. A. 15°, Dec. + 30°. The radiant space was very large, and seemed to me oblong—say 50° by 37°. The space which I marked upon the globe was enclosed in an ellipse having its axes terminating as follows:

Major axis from R. A. 347°, Dec. +20°, to R. A. 42°, Dec. + 40°
Minor " " 5° " 47°, " 16° " 12°

"Of course these figures cannot make claim to much accuracy, but I record my impression, noted at the time. I also noted as follows: paths short—not many over 5°; color of larger ones yellowish; velocity moderate; paths of 3 or 4 appeared wavy; brilliancy quite moderate, few if any equal to a star of 1st mag.; some few with trains, but *none* of them *persistent*. On Thursday and Friday evenings not a meteor was to be seen."

10. *In Haddonfield, N. J.*—Mr. W. C. Taylor, of Philadelphia, writes: "From the numbers seen by myself, and several members of my family, I am satisfied that 20 per minute (for 5 observers) is a safe estimate of their frequency at seven o'clock Supposing that, as with the earlier meteors of this month, the display would intensify as the night advanced, I did not keep a continuous watch at this time. Toward eight o'clock our sky became partially overcast, but there remained visible enough of the heavens to show that the number of meteors had greatly lessened. At ten o'clock the intervals averaged about 30 seconds for one observer. At two o'clock this (Thursday) morning, in two short watches, I saw not a single meteor."

"I have been an observer of the November meteors for many years, but never, except on one occasion, saw them so abundant as they were early last evening. I can name no better radiant than γ Andromedæ."

11. *In Oxford, Conn.*—Mr. O. Harger was on the road to Oxford at 6^h, P. M., Wednesday. In what he estimated as 10 minutes he counted, alone, 100 meteors. The period included the time of striking the hour *six*.

From 8^h 3^m P. M. to 8^h 22^m, he and his brother counted 100 meteors.
" 8 22 " 8 39 " " 50 "
" 9 16 " 9 27 " " 11 "

12. *In Indiana.*—At Greencastle, Ind., Prof. Tingley counted 110 in 40 minutes, at a time not later than 7^h 55^m, P. M. At Princeton from 7^h 45^m to 8^h 15^m, P. M., Mr. Hunter and others in one half of the sky saw 70.

Remarks upon the Display.

Dr. Weiss, of Vienna, who first pointed out in 1868,* the probable connection between Biela's comet and the meteors seen

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* Sitzungsberichte, vol. lvii.

c. 6th, 1798, by Brandes, and Dec. 6th, 1838, by Mr. Herrick,
es the radiant for meteors following the path of that comet,
R. A., 23°·4, N. Decl., 43°·0. I assigned a point 3° from
Andromedæ as the center of the radiant of the meteors, or
out R. A. 25°·3, N. Decl., 43°·3. The longitude of the node
Biela's comet was in 1852, according to Hubbard, 245° 51′,
d the comet would pass about a million of miles from the
rth's orbit, between it and the sun. We passed that place of
node early Wednesday evening, Nov. 27th. There can
rdly be a doubt therefore that these meteors were once frag-
nts, or companions, of that comet.

Any theory that shall explain the formation of the present
ouping of meteoroids must account for the magnitude and
ape of the radiant areas. If the members of a group have
arly the same orbit, the radiant should be a point. But the
a of the radiant, Nov. 24–27th, was at least 8° long. This
plies that the orbits differ considerably, either,—(*a*) in their
linations to the ecliptic; (*b*) in their major axes; (*c*) in
longitude of perihelion; or, in two or three of these ele-
nts combined.

The shower ended abruptly Wednesday evening, and in the
ar evenings that followed nothing special was to be seen.
nilarly marked limits are not uncommon in other showers.
e orbits must then either lie approximately in a plane or
re must be a common node in the ecliptic, where the earth
ets them. Such a node would point unequivocally to the
th as the body that originally scattered the comet.

If, as seems more probable, the orbits, however, lie nearly in
plane, either the major axes, or the longitudes of the peri-
ia, must differ widely. Neither of these conditions could be
isfied, so far as I can see, by a group formed from the dis-
sion of a comet by Jupiter, or other large planet. If the
gments of the comet leave the neighborhood of Jupiter, they
uld after each revolution return nearly to the same point in
ce. But a radiant area 8° or 10° long on the night of Nov.
h, implies a distribution of the aphelia over 10° or 12° of
gitude, or a similarly large difference of major axes. Such
its can hardly have a common point at a great distance from
sun. Moreover, a scattering accomplished in a short time
on a body moving in an orbit inclined several degrees to the
iptic should, it would seem, be incompatible with a group-
at the earth's node.

Again, suppose a disrupted body or agglomeration, has been
e changed into a stream by the differential action of gravita-
n in the manner shown so beautifully by Schiaparelli. If
perturbing forces exerted by any planet or planets, whether
nporary or long continued, should produce such differences

of major axes, or longitudes of perihelia, by *differentiul* action, the total action would, undoubtedly, entirely scatter the group at the earth's nodes.

In fact, instead of regarding the meteors as a stream we ought rather to look upon the group as coming together near the perihelion,—or near the node,—and then scattering widely, to reassemble, perhaps, after a complete revolution in the orbit.

A resisting medium cannot account for the observed effect, for this does not change the longitude of the perihelion of an orbit.

It seems to me, therefore, that the periodic meteors cannot have been brought into the solar system as a stream, but that the forces which have scattered the comets are those acting near the perihelia of their orbits. As a probable corollary, we may infer that whatever force divided Biela's comet into its two principal parts was one acting near the perihelion.

If we consider the orbits of the meteors of Nov. 14th, the preceding discussion is simplified. That shower is sharply limited, being in its greatest intensity only one or two hours long. Its recurrence at regular intervals of one-third of a century, for nearly a thousand years, precludes great differences of the major axes of the individual orbits, and the secular procession of the node of the group, *as a group*, equally forbids great differences of inclination of the orbits.

The size of the radiant is therefore due almost exclusively to the difference of the longitudes of the perihelia. This difference for that group cannot be less than 15°.

In conclusion I would say that we have no evidence, as yet, that any radiant of meteors is so small as is apparently required by the supposition of the distinguished Italian astronomer, that the meteors were drawn as a stream into the solar system from the stellar spaces. With Prof. Weiss and others, I am inclined to consider them all to have been once connected with periodic comets. The scattering took place apparently at or near the perihelion.

ART. VIII.—*Discovery of a new Planet;* by JAMES C. WATSON.

(From a communication to one of the Editors.)

ON the 25th inst. at $7^h\ 30^m$ I discovered in Taurus a planet hitherto unknown. It is large and bright, resembling a star of the 9th magnitude. The unfavorable state of the weather has prevented me from obtaining any observations later than the 26th. The following are the places observed:

1872.	Ann Arbor m. t.	α	δ
Nov. 25,	$9^h\ 49^m\ 31^s$	$4^h\ 21^m\ 44^s{\cdot}92$	$+19^\circ\ 34'\ 16''{\cdot}2$
25,	10 21 51	4 21 43·44	19 34 19·9
25,	10 47 14	4 21 42·65	19 34 18·0
26,	11 9 4	4 20 40·72	+19 34 39·7

Observatory, Ann Arbor, Nov. 30, 1872.

SCIENTIFIC INTELLIGENCE.

I. CHEMISTRY AND PHYSICS.

Tests for certain Organic Fluids.—In his investigations con-
l with the ammonia process of water-analysis, WANKLYN
ved: (1) that when an animal fluid is mixed with an excess
ash, evaporated down in contact with the alkali, and main-
l at a temperature of 150° C. for some time, it evolves a
n fixed proportion of ammonia; (2) that after this, a further
ity of ammonia may be obtained by boiling the residue with
sium permanganate in alkaline solution. He also observed
for certain animal fluids, not only is the total quantity of
nia obtained characteristic, but the relative amounts yielded
e two processes given are so also. This fact, Wanklyn
s, will be of value in practical biology and medical juris-
nce. For example, he has proved that urine yields a large
rtion of ammonia to potash (the only fluid that does so), and
ll quantity to permanganate; milk yields half as much to pot-
s to the permanganate; blood yields one-fifth as much; moist
of egg about one-fourth as much; gelatin gives no ammonia
ly the least trace) to potash, but a good quantity to the per-
anate. In the experiments, 5 c.c. of the animal fluid were
d to 500 c.c., and 5 or 10 c.c. taken for each analysis. The
l was heated in a minute retort connected with a small
g's condenser, on an oil bath; and the ammonia determined
essler's test. By this process, it is possible to distinguish
actorily between a spot of milk and one of white of egg upon
nbric handkerchief.—*Jour. Chem. Soc.*, II, x, 645, August,

G. F. B.

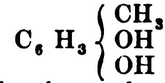
On the Transformation-products of Starch.—The results of
ction of malt-extract upon starch, as given by different
rs, being discrepant, O'SULLIVAN has reinvestigated the
ct, examining both of the products, (1) the dextrin and (2)
ugar. For the preparation of the former, 100 grams air-
starch were stirred up with 300 c.c. of water at 40° and the
re poured into 2 liters of boiling water. After the paste
cooled to 70° C., the cold extract from 20 grams pale malt
dded to it. When the solution was no longer colored by
, it was boiled, cooled, filtered, evaporated to 300 c.c., and
itated by alcohol. Similar quantities of paste were trans-
d by the action of sulphuric and of oxalic acids. The white,
mass, even after 30 precipitations, still retained the power
lucing cupric oxide to the extent of 8 or 9 per cent. of dex-
To eliminate this reducing body, the dextrin solution was
itted to fermentation, and again precipitated by alcohol, three
r times. It was finally washed with alcohol, thrown on a
pressed in bibulous paper, and dried over sulphuric acid.
preparations made by these general methods, modifying

only the details, gave products remarkably uniform in character and composition, but still retaining a reducing power equal to nearly two per cent. dextrose. Pure dextrin thus obtained is a brittle white powder, showing shining fracture-surfaces, easily soluble in water, not perceptibly so in cold alcohol of sp. gr. 0.82. An aqueous solution, containing 10 grms. in 100 c.c., has a sp. gr. 1.0385. Its specific rotatory power is $[a] = +213°$. Dried over sulphuric acid, the weight becomes constant when it contains 9.5 to 10 per cent. of water. This it loses in a current of dry air at 100°. Analysis of the dextrin dried over sulphuric acid gives the formula $C_6H_{10}O_5 + H_2O$. Treated with malt-extract its solution increases in reducing power, and becomes constant when the amount of sugar present, calculated as dextrose, is equal to 66 per cent. of the dextrin employed. The specific rotatory power is then $[a] = 150°$. To study the sugar produced, a starch paste, made as above, was treated with the extract from 20 grams pale malt, and the mixture allowed to stand at 40° to 45° for three hours. It was boiled, cooled, filtered, evaporated to 300 c.c., boiled with 2 liters of alcohol (sp. gr. 0.82), cooled, the alcohol decanted and set aside. In six days the sides of the vessel were covered with a crystalline crust. Five other preparations of this substance were made, some from the mother-liquors, others by dialysis and reprecipitation. The products were all alike, had a specific rotatory power $[a] = +150$, and reduced copper oxide equal to 65 per cent. glucose. Analysis gave numbers agreeing with the formula $C_{12}H_{22}O_{11}$. The author believes, therefore, that the end-product of the action of malt-extract upon starch is a sugar analogous to lactose, which has a reducing power only two-thirds as great as dextrose, and which appears to be identical with Dubrunfaut's maltose.—*Jour. Chem. Soc.*, II, x, 579, July, 1872. G. F. B.

3. *On Dextrin.*—Considering dextrin as the result of the union of two molecules of dextrose by the elimination of one molecule of water, MUSCULUS has attempted its synthesis. By solution of dextrose in sulphuric acid, sulpho-dextrosic acid was obtained. Instead of breaking this up by heat, as in the ordinary etherification process, Musculus dissolved it in alcohol of 95 per cent.—in which dextrin is insoluble—filtered the solution, and placed it aside. The next day a slight precipitate appeared, which increased daily for three weeks. The supernatant liquid was decanted; the precipitate, washed free from sulphuric acid by alcohol, was dissolved in water and evaporated to dryness. A dry, friable, and almost colorless amorphous mass was obtained, which had no sweet taste, was very soluble in water, insoluble in alcohol, did not reduce the copper test, was saccharified feebly by diastase, not colored by iodine, converted into dextrose by heating with dilute sulphuric acid, and having a rotatory power nearly double that of dextrose. Agreeing in all except the last particular, closely with dextrin.—*Bull. Soc., ch.* II, xviii, 66, July 15, 1872. G. F. B.

4. *On the existence of an inferior Homologue of Benzol.*—Kekulé's theory of the hexacarbon nucleus of aromatic compounds forbids the existence of an inferior homologue of benzol. The announcement of Carius that he had obtained pentene C_5H_4, which would be such a homologue, is of so much importance that the question of its true character should be at once determined. Rommier has submitted, therefore, the lightest products of coal-tar to examination, with a view of detecting pentene. The first product isolated was a small quantity of an oil boiling between 40° and 50°, which had the density of water and consisted of nearly pure carbon disulphide. A second product, boiling from 58° to 62°—pentene, according to Carius, boiling at 60°—after the separation of the CS_2, was nitrated, and yielded a compound identical with binitrobenzol. Pentene has no existence in coal-tar, therefore.—*Bull. Soc. Ch.*, II, xviii, 70, July 15, 1872. G. F. B.

5. *On the Synthesis of Orcin.*—Orcin, discovered by Robiquet in 1829, is the basis of the coloring matter of lichens. It is a homologue of resorcin, and has the formula



$C_6\,H_3\begin{cases}CH_3\\OH\\OH\end{cases}$

Vogt and Henninger have succeeded in producing it synthetically. By the action of chlorine upon toluol containing a little iodine, chlortoluol, boiling between 157° and 159°, was obtained. This, heated with two or three times its weight of sulphuric acid, yielded a thick brown liquid containing two isomeric acids named respectively α and β chlorocresylsulphurous acids. They were separated by the greater solubility of the barium salt of the latter. The α chlorocresylsulphite of potassium obtained from the barium salt, was fused in a silver capsule with twice its weight of potassium hydrate. Hydrogen was disengaged, and, after cooling, the mass was dissolved in water, acidulated with hydrochloric acid, agitated with benzol to remove small quantities of salicylic acid and cresylol formed in the reaction, and then with ether to dissolve the orcin. After removal of the ether by distillation, a brown syrupy residue was left which was dissolved in water, filtered, evaporated to dryness in a current of carbonic gas, and distilled in vacuo. A yellow, thick liquid passed over between 185° and 190°, which solidified on cooling. This, on solution in water and recrystallization either from water or chloroform, gave crystals of pure orcin, identical in all its properties with the natural product.—*Bull. Soc. Ch.*, II, xvii, June, 1872.

G. F. B.

6. *On a simple method of determining Molecular Weights from the Vapor Volume.*—Instead of ascertaining the absolute vapor-density, now of little use, Landolt proposes to determine only the relative vapor-density. Inasmuch as the molecular weights of volatile bodies represent the weights of equal volumes, it is sufficient to take two tubes of equal size, to place in one a substance of well-known molecular weight, as 18 mgr. of water, or better 119.5 mgr. of chloroform, and in the other a quantity of the

substance to be examined equal to its molecular weight in milligrams. These tubes, as in Hofmann's method for vapor-densities, are placed in the same mercury cistern and surrounded with a glass cylinder, into which the vapor of water, or of some substance whose boiling point is higher, may be conducted. If the assumed molecular weight be correct, the mercury will stand at the same level in both tubes; if not, it must be either half or twice the assumed weight, in which case a marked deviation from uniformity will be observed. It is obvious that by this method, all corrections for temperature and pressure, and the tension of the mercury vapor, and all the subsequent calculation, are avoided. The only difficulty seems to lie in the accurate weighing; but, practically, this is no objection, since the weighing of liquids is effected in small bulbs, and it is sufficiently accurate if carried to milligrams. The tubes are calibrated easily by pouring into each of them a weighed quantity of mercury, about equal in volume to that of the vapor to be estimated. If the mercury does not stand at the same height in both, gradually push a glass rod into it in the larger tube until the same level is reached. Mark carefully the point to which the rod is immersed, cut it off at this place, and allow the piece to rise to the top of the mercury in that tube before heating. By using six of these tubes arranged in one cistern and in a convenient frame, the uniformity of vapor tension when molecular weights of different liquids are used, may be excellently well shown as a lecture experiment.—*Ber. Berl. Chem. Ges.*, v, 597, June, 1872. G. F. B.

7. *On the combined action of Heat and Pressure on Paraffin.*—In a second paper on this subject, THORPE and YOUNG give the results of distillation under pressure of 3½ kilos. of paraffin made from shale, which melted at 46° and was composed of C 85.14, H 14.81. About 4 litres of fluid hydrocarbons were obtained, of which 0.3 liter boiled below 100°, 1 liter between 100° and 200°, and 2.7 litres between 200° and 300°. The marsh gas series and the olefine series—no others were present—were separated by means of bromine. Of the former, pentane, hexane, heptane, octane, and nonane were obtained. Of the latter, the brominated derivatives of the corresponding olefines. Paraffin undergoes a similar decomposition to butane and other members of the marsh gas series.—*Ber. Berl. Chem. Ges.*, v, 556, July, 1872. G. F. B.

II. GEOLOGY AND NATURAL HISTORY.

1 *The Eastern Limit of Cretaceous Deposits in Iowa;* by C. A. WHITE, of Iowa City, Iowa. (Read before the Dubuque Meeting of the American Association for the Advancement of Science.)—At the Chicago meeting of this Association, I had the honor to announce the existence of Cretaceous strata in Guthrie county, Iowa, where they rest unconformably upon the Coal measures; the locality being about eighty miles eastward from the Missouri river, and about forty miles west of the city of Des Moines. I

, subsequently, examined Cretaceous strata in Brown and wood counties, in southwestern Minnesota, where they rest nformably upon rocks of Azoic age. So far as I am aware, e are the most easterly localities in the interior region of th America at which strata of Cretaceous age have been ally observed *in situ*. The first mentioned Cretaceous rocks referred to the division which, in my report on the Geology owa, I have named the Nisnabotany sandstone, and the latter he division called the Inoceramus beds, in the same report. these, as well as all the Cretaceous rocks hitherto know in the rior region, eastward from eastern Nebraska and Dakota, are rred to the "Earlier Cretaceous" of Meek and Hayden.

have now to announce discoveries of Cretaceous fossils, and ments of strata containing them, in the drift of Iowa at other ts much farther eastward; the collection which has been made ifferent localities containing specimens which belong to seve- f the most characteristic types of that period, especially of its r epochs.

uring the year 1870 my attention was called to the existence hese fossils in the drift of Howard county, Iowa, by Mr. John mith, of Lime Springs, and a few weeks ago I visited the local- indicated, in company with him. It is found in a railroad cut northwest of the village, which is less than five miles south of northern boundary of Iowa. The fossils and fragments of ta are found imbedded in the ordinary compact blueish clay of unaltered drift, twenty or twenty-five feet beneath the surface he soil. The collections have not yet been critically studied, the following statement of the one made at Lime Springs will a general idea of its character: 1. Squaloid teeth, of the us *Otodus;* 2, Teeth of *Saurocephalus?;* 3. Bones, teeth and es of teliost fishes; 4. *Belemnitella;* 5. *Ammonites* (2 spe-); 6. *Natica??;* 7. *Dentalium;* 8. *Ostrea;* 9. *Inoceramus;* *Leda?;* 11. *Cytherea;* 12. *Corbula.*

r. P. McIsaac, of Waterloo, Blackhawk county, Iowa, has ly found a *Belemnitella* in the unaltered drift near that city, ddition to the ammonite he found there a few years ago. The *mnitella* is of the same species as those found in Howard ty, sixty miles directly northward.

he fish teeth have been submitted, for examination, to my d Prof. O. H. St. John, who writes me as follows concerning n.

All the squaloid teeth belong to the genus *Otodus* of Agassiz, may represent three species, but I suspect they are but so y forms of one species; this relationship can be determined by examination of a much larger suite of specimens, since the h vary so much in shape and size from different portions of the . I have not been able to determine their specific identity, gh they are somewhat like *O. appendiculatus* Ag., a form inally made known from the European chalk formation, and which later Cretaceous and Tertiary teeth from this country have been identified—I do not presume to say upon what authority. With *some* of the latter your specimens are intimately related, perhaps identical. You have two or three fragments of teeth (one nearly perfect), which probably belong to the same genus as those from the New Jersey Greensand and *later* deposits known as *Saurocephalus.* All these teeth evidently belong to a later epoch than the chalky beds on the Big Sioux river, near Sioux City, the fishes of which have a much stronger resemblance to those from the Chalk of Europe than have the specimens under consideration, while the squaloid teeth among the latter bear the most intimate resemblance to certain forms of *Otodus* from the Cretaceous rocks of Alabama. Hence I conclude your specimens have been derived from deposits of the Later Cretaceous, probably equivalent to the Alabama fish-bearing Cretaceous strata. That they are *very* late Cretaceous forms there can be no doubt, from the fact of their close relationship to teeth found in the Eocene of the Old World. I am not prepared to show how close this relationship is, although the first sight of your little collection strongly suggested their Eocene age."

Although all the specimens forming the subject of this memoir have been found in the drift, they have been found at such localities, and under such circumstances as to leave no doubt in the mind of the writer that the Cretaceous sea once extended as far eastward, between the 42d and 44th parallels of latitude, as the 92d degree of longitude west from Greenwich. This is nearly two hundred miles further eastward than any Cretaceous deposits were ever known to have extended in the interior region of North America at the time I commenced my official examination of the geology of Iowa in 1866. What gives additional interest to these discoveries is the fact that the fossils doubtless belong to a Mesozoic epoch as late as any yet recognized in any part of North America, and much later than that of any Cretaceous strata of Iowa or of the adjacent parts of Nebraska and Dakota hitherto known. It is true the deposition of *late* Cretaceous deposits only, in the region indicated, requires the assumption that a subsidence took place there during that period; but a similar condition of other strata is found in southwestern Minnesota, where the Inoceramus beds, as before stated, rest upon the Azoic rocks, the older Nishnabotany sandstone being absent there, but present about 150 miles to the southwestward.

None of the strata in which these fossils were originally deposited have, as before intimated, been found *in situ*, but fragments of them, and also the material of the drift to which they evidently, in part, gave origin, show that they were soft and friable like most of the Cretaceous rocks of the great interior region. Consequently they were readily disturbed and removed by the forces in operation during the Glacial epoch. While much of the material of these strata was doubtless transported to great distances and its character as such thus obliterated, delicate fossils, as well as soft and friable fragments of the strata, are found embedded in the velly clay so slightly eroded as to forbid the belief that they e been transported to any considerable distance from the place heir origin.

he fragments of strata referred to have been recognized, so far, y at Lime Springs, but their presence there, as well as the con- ion in which they are found, inspires the confident hope that may yet find some of these Cretaceous strata *in situ* in that inity.

hese discoveries also suggest that we should scan more closely n ever before, not only the character and contents of the drift central and eastern Iowa, bnt also some of the strata of the e regions, especially sandstones, to determine with certainty ether some of them may not be of Mesozoic age.

. *On Actual Glaciers in California;* by JOHN MUIR.— one of the yellow days of October, 1871, when I was among mountains of the "Merced group," following the foot-prints of ancient glaciers that once flowed grandly from their ample ntains, reading what I could of their history as written in raines, cañons, lakes, and carved rocks, I came upon a small eam that was carrying mud of a kind I had never seen. In a m place, where the stream widened, I collected some of this d, and observed that it was entirely mineral in composition, l fine as flour, like the mud from a fine-grit grindstone. Before ad time to reason, I said, "Glacier mud—mountain meal!"

hen I observed that this muddy stream issued from a bank of h quarried stones and dirt, that was sixty or seventy feet in ght. This I at once took to be a moraine. In climbing to the of it, I was struck with the steepness of its slope, and with its , unsettled, plantless, new born appearance. The slightest ch started blocks of red and black slate, followed by a rattling in of smaller stones and sand, and a crowd of dry dust of mud, whole moraine being as free from lichens and weather-stains f dug from the mountain that very day.

hen I had scrambled to the top of the moraine, I saw what med to be a huge snow-bank, four or five hundred yards in gth, by half a mile in width. Imbedded in its stained and fur- ed surface were stones and dirt like that of which the moraine s built. Dirt-stained lines curved across the snow-bank from e to side, and when I observed that these curved lines coincided h the curved moraine, and that the stones and dirt were most ndant near the bottom of the bank, I shouted "*A living cier!*"

hese bent dirt-lines show that the ice is following in its dif- nt parts with unequal velocity, and these imbedded stones are rneying down, to be built into the moraine, and they gradually ome more abundant as they approach the moraine, because re the motion is slower.

n traversing my new-found glacier, I came to a crevasse, down ide and jagged portion of which I succeeded in making my , and discovered that my so-called snow-bank was clear, green ice, and, comparing the form of the basin which it occupied with similar adjacent basins that were empty, I was led to the opinion that this glacier was several hundred feet in depth.

Then I went to the "snow-banks" of Mts. Lyell and McClure, and, on examination, was convinced that they also were true glaciers, and that a dozen other snow-banks seen from the summit of Mt. Lyell, crouching in shadow, were glaciers, living as any in the world, and busily engaged in completing that vast work of mountain-making accomplished by their giant relations now dead, which united and continuous, covered all the range from summit to sea.

But, although I was myself thus fully satisfied concerning the real nature of these ice masses, I found that my friends regarded my deductions and statements with distrust; therefore, I determined to collect proofs of the common, measured, arithmetical kind.

On the twenty-first of August last, I planted five stakes in the glacier of Mt. McClure, which is situated east of Yosemite Valley, near the summit of the range. Four of these stakes were extended across the glacier, in a straight line, from the east side to a point near the middle of the glacier. The first stake was planted about twenty-five yards from the east bank of the glacier; the second, ninety-four yards; the third, 152, and the fourth, 225 yards. The positions of these stakes were determined by sighting across from bank to bank, past a plumb-line, made of a stone and a black horse-hair.

On observing my stakes on the sixth of October, or in forty-six days after being planted, I found that stake No. 1, had been carried down stream eleven inches; No. 2, eighteen inches; No. 3, thirty-four, and No. 4, forty-seven inches. As stake No. 4 was near the middle of the glacier, perhaps it was not far from the point of maximum velocity—forty-seven inches in forty-six days, or one inch per day. Stake No. 5 was planted about midway between the head of the glacier and stake No. 4. Its motion I found to be, in forty-six days, forty inches. Thus these ice-masses are seen to possess the true glacial motion. Their surfaces are striped with bent dirt-bands, and are bulged and undulated by inequalities in the bottom of their basins, causing an upward and downward swegding, corresponding to the horizontals wedging as indicated by the curved dirt-bands.

The Mt. Mc Clure glacier is about one-half of a mile in length, and the same in width at the broadest place. It is crevassed on the south-east corner. The crevasse runs about south-west and north-east, and is several hundred yards in length. It is nowhere more than one foot in width.

The Mt. Lyell glacier, separated from that of McClure by a narrow crest, is about a mile in length. I have planted stakes in the glaciers of "Red Mountain," also, but have not yet observed them.

The Sierras adjacent to the Yosemite Valley are composed of slate and granite, set on edge at right angles to the direction of the range, or about north 30 deg. east, and south 30 deg. west. Lines of cleavage cross these, running nearly parallel with the main range; and the granite of this region has a horizontal cleavage or stratification. The first mentioned of these lines have the fullest development, and give direction and character to many valleys and cañons, and determine the principal features of many rock forms. No matter how hard, how domed or homogeneous the granite may be, it still possesses these lines of cleavage, which require only simple conditions of moisture, time, etc., for their development. But I am not ready to discuss the origin of these planes of cleavage, which make this granite so easily denuable, nor their full significance with regard to mountain structure in general. I will only say here, that oftentimes the granite contained between two of these north 30 deg. east planes is softer than the rock outside, and has been denuded, leaving vertical walls, as determined by the direction of the cleavage, thus giving rise to those narrow-slotted cañons, called "devil's lanes," "devil's gateways," etc.

In many places, in the higher portion of the Sierras, these slotted cañons are filled with snow, which I thought might prove to be ice; might prove to be living glaciers, still engaged in cutting into the mountains, like endless saws. To decide this question, on the 23d of August last, I set two stakes in the narrow-slot glacier of Mt. Hoffman, marking their position by sighting across from wall to wall, as I did on the McClure glacier; but on visiting them, a month afterward, they had been melted out, and I was unable to decide anything with any great degree of accuracy.

On the 4th of October last, I stretched a small trout line across the glacier, fastening both ends in the solid banks which at this place were only sixteen feet apart. I set a short, inflexible stake in the ice, so as just to touch the tightly-drawn line, by which means I was enabled to measure the flow of the glacier with great exactness. Examining the stake in twenty-four hours after setting it, I found that it had been carried down about three-sixteenths of an inch. At the end of four days, I again examined, and found that the whole downward motion was thirteen-sixteenths of an inch, showing that the flow of this glacier was perfectly regular.

In accounting for those narrow-lane cañons, so common here, I always referred them to ice-action in connection with special conditions of cleavage, and I was gratified to find that their formation was still going on. This Hoffman glacier is about 1,000 feet long by fifteen to thirty feet wide, and perhaps 100 feet deep in the deepest places.

I go back to the mountains to complete these observations. These are the first fruits, and the rest of the crop I will bring in when I come to study in the Coast Range.—*Overland Monthly for December.*

3. *Return of the Yale College Geological Expedition.*—Professor **Marsh** and party returned on the 7th of December from the Rocky

Mountains, where they have spent the last two months in geological researches. They bring back a large number of vertebrate fossils from the Cretaceous and Tertiary formations of the West, including many new and interesting Mammals, Birds and Reptiles. Among the treasures secured during the present trip was a nearly entire skeleton of *Hesperornis regalis* Marsh, the gigantic diving bird of the Cretaceous; a second species of *Ichthyornis* (*I. celer* Marsh), and numerous remains of Pterodactyls. The new fossils will soon be described by Professor Marsh.

4. *On Spontaneous fission? in Zaphrentis.*—(Communicated to this Journal).—Among the specimens of *Z. spinulifera* Hall, collected from the St. Louis limestone in Marion County, Iowa, is one showing what is probably true spontaneous fission. This is the only specimen showing such a character, among thousands of specimens of many species, which I have collected from the Palaozoic rocks of America, and seems remarkable in a genus so distinctively, simple and solitary. The outline of the specimen is such as to faintly suggest that its peculiarity may be the result of a fusion of two individuals, but not only is there no limiting wall dividing it into two parts, there is also no impression or suture of the epitheca to suggest such a fusion. The specimen is of ordinary height, the outline of the double calyx being oblong, one diameter being about twice as great as the other. There are two well marked septal fossettes, forming an angle of about 130 degrees with each other, and the other parts are developed in the ordinary way, the transverse plates being common to both parts.

The peculiar form of this specimen is evidently not the result of violence, because the epitheca and transverse plates are unbroken. Specimens showing complete recovery from violence, inflicted while the polyp was living, are not uncommon.

Iowa State University, Aug. 7th, 1872. C. A. WHITE.

5. *Volcano of Kilauea.*—From one of our residents who has just returned from Kilauea by the *Annie*, we learn that the crater is very active. The old South Lake is full and running over in two broad streams, one to the south and the other to the west. A number of beautiful cones were in action, and sending up continuous jets of lava. Mr. Jones, the proprietor of the Volcano house, describes the scene as finer than any he has seen for years. During the last two weeks a number of slight earthquake shocks have been felt at Kapapala, and on Oct. 13th a very heavy one was felt at Hilo—the heaviest that has been felt since the great shock of April, 1868.—*Hawaiian Gazette, Oct.* 27.

6. *The Physical Geology and Geography of Great Britain;* by A. C. RAMSAY, LL.D., F.R.S., Director-General of the Geological Surveys of the United Kingdom. 3d edition, 350 pp. 12mo. With a geolocical map printed in colors. London. (Edward Stanford.)—It is not surprising that Prof. Ramsay's Physical Geology and Geography of Great Britain should have great popularity at home. And it also may well have in this country. For British facts do not belong to Britain, but to

the world, and illustrate universal principles. The general subjects of the origin and arrangements of rocks, and their upturnings and metamorphism; the origin of mountains and valleys and the general features of the surface; and the action of water in the condition of oceans, rivers and glaciers, are treated with a clearness and simplicity calculated to attract the popular reader. The succession of life on the globe is only touched upon briefly with reference to important general truths. But the facts connected with the more recent life in Britain, especially that of the cave animals, and of other Quaternary deposits in which occur traces of man along with bones of extinct quadrupeds, occupy the principal parts of two chapters; and in connection, such subjects as the separation of Britain from the continent, the origin of various rivers, valleys, river terraces and raised beaches, the influence of changes of level on the features and courses of rivers of Britain and France, the qualities of river waters and the nature of British soils, come in for consideration. The work closes with a chapter on the relation of the different races of men in Britain to the geology of the country; and another on the industrial products of the geological formations of Britain, the quantities of available coal in the coal-fields, and the probability of finding other coal-fields beneath the more recent formations. Most of the subjects are illustrated by new facts and views drawn by the author from his own extensive observations, and the volume is thereby rendered a work of great importance to all geologists. Its value is much enhanced by a geological map of Great Britain beautifully colored, which forms its frontispiece.

7. *Elemente der Geologie*, von Dr. HERMANN CREDNER, Professor an der Universität Leipzig. 538 pp. with 380 woodcuts. Leipzig, 1872. Wilhelm Engelmann).—Dr. Credner's work is a well arranged and well illustrated brief manual of geology. The author, some years since, spent considerable time on this continent making geological observations, and has drawn his illustrations more from American facts than is common with German treatises. We think there is much that is hypothetical in his views on the American Huronian, but in general his remarks on American geology are trustworthy.

8. *Lithologie des Mers ;* by M. ACHILLE DELESSE.—The publication of the maps of Delesse's new work has already been noticed in this Journal. The text has recently appeared. The author considers in succession the lakes and seas of the world, describes the nature of the bottom and shores, gives lists of the species characterizing them, and found along their coasts, and points out the geological bearing of these and other facts connected with this subject. Delesse has here taken the initial step in an important department of geological science, and has gathered together a large number of facts of interest. At the same time the work shows us how little we know with regard to this great subject. Some additional facts might have been introduced with reference

to the bed of the deep ocean; but the ocean's bottom is still to a great extent an unknown region, and much more investigation will be required before the facts connected with the inland waters of the continents can be properly systematized. Delesse by his labors has given increased interest to these investigations.

9. *Notice of a new Species of Ichthyornis;* by O. C. MARSH.—A second species of the peculiar genus of Cretaceous birds, with biconcave vertebræ (*Ichthyornis*), was found by the writer during a recent visit to Western Kansas. The remains indicate a bird rather larger than *Ichthyornis dispar* Marsh, but of more slender proportions. It may readily be distinguished from that species by the sacrum, which is proportionally more elongated, and has the cup of the posterior vertebral face more deeply concave. This species may be called *Ichthyornis celer*, and the group of birds now represented by the two species may be named *Ichthyornidæ*.

Yale College, Dec. 16th, 1872.

10. *Recherche de la Rapidité de Croissance des Bancs de Coraux dans l' Ocean Pacifique; Expérience faite à l'île de Taïti, en Novembre*, 1869; par MM. F. LE CLERC, Lieutenant de vaisseau, et DUHIL DE BÉNAZÉ, Ingenieur des constructions navales, pp. 22, 8vo. Paris, 1872. (Adolphe Lainé, rue des Saints-Pères, 19).—After some general observations of interest, the authors of this pamphlet give an account of their attempt to ascertain the rate of growth of the coral reef at Tahiti, called the Dolphin Shoal, by measurements from the level of the stone planted on the shore on Point Venus, by Capt. Wilkes, and comparing their results with his. They made measurements: but they observe that Wilkes does not state whether he measured from the top of a head of coral or from the solid bank on which the corals were growing, and further, that the use of an "excellent spirit level," from a stone of so little length, is not sufficiently exact for correct results. Hence, they draw no conclusion from their results. Before leaving the region, they made the following arrangements with reference to future measurements. They planted two blocks of coral, cementing them below and nearly burying them in the soil, placing them 0·21 meters above the Wilkes stone which is between them; they then put a mark upon them on plates of metal, directed toward the place of observation on the shoal. A third stone was placed 40 meters from the southwest angle of the Point Venus lighthouse, in order to give a second observation on the position of the spot on which soundings were to be made. This spot was found to bear from the two new stones N. 77° 30′ E.; from the third stone N. 70° 55′ E.; from the bell of the new mission church S. 81° 40′ E. A horizontal line passing from the mark on the new stone is 7·460^{m} above the madreporic heads. This observation they leave for comparison with future measurements. They observe that the principal coral of the bank is the *Madrepora plantaginea*. They farther made observations that satisfied them that Tahiti was not at present undergoing any general elevation. Two maps accompany the pamphlet; one is copied from Wilkes; the other is from a chart by MM. Le Clerc and Minier, lieutenants of

the world, and illustrate universal principles. The general subjects of the origin and arrangements of rocks, and their upturnings and metamorphism; the origin of mountains and valleys and the general features of the surface; and the action of water in the condition of oceans, rivers and glaciers, are treated with a clearness and simplicity calculated to attract the popular reader. The succession of life on the globe is only touched upon briefly with reference to important general truths. But the facts connected with the more recent life in Britain, especially that of the cave animals, and of other Quaternary deposits in which occur traces of man along with bones of extinct quadrupeds, occupy the principal parts of two chapters; and in connection, such subjects as the separation of Britain from the continent, the origin of various rivers, valleys, river terraces and raised beaches, the influence of changes of level on the features and courses of rivers of Britain and France, the qualities of river waters and the nature of British soils, come in for consideration. The work closes with a chapter on the relation of the different races of men in Britain to the geology of the country; and another on the industrial products of the geological formations of Britain, the quantities of available coal in the coal-fields, and the probability of finding other coal-fields beneath the more recent formations. Most of the subjects are illustrated by new facts and views drawn by the author from his own extensive observations, and the volume is thereby rendered a work of great importance to all geologists. Its value is much enhanced by a geological map of Great Britain beautifully colored, which forms its frontispiece.

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nitrogen when compared with it appeared double, and each component nebulous and broader than the line of the nebula. This latter line was seen on several nights to be apparently coincident with the middle of the less refrangible line of the double line of nitrogen. This observation was on one night confirmed by observation with the more powerful spectroscope. . . . . . I have not yet been able to find a condition of luminous nitrogen in which the line has the same characters as those presented by the line in the nebula, when it is single and of the width of the slit. . . . . Upon the whole, I am inclined to regard the line in the nebula as probably due to nitrogen.

"*Second line.* This line was found by my former comparisons to be a little less refrangible than a strong line in the spectrum of barium. These sets of measures give for this line a wave-length of 4957 on Angström's scale; this would show that the line agrees nearly in position with a strong line of iron. At present I am not able to suggest to what substance this line belongs. . . . . This line is also narrow and defined. I suspect that the brightness of this line relatively to the first line varies in different nebulæ.

"*Third and fourth lines.** My former observations show that these lines agree in position with two lines of the spectrum of hydrogen, that at F and the line near G. These lines are very narrow and are defined. . . . I have not been able to obtain decisive observations as to the possible motion of the nebula in the line of sight.

In his paper Dr. Huggins gives the details of his observations on various stars, and the following are his results tabulated.

TABLE I.—*Stars moving from the Sun.*

Star.	Compared with	Apparent motion in miles.	Earth's motion.	Motion from Sun.
Sirius,	H	26 to 36	—10 to 14	18 to 22
Betelgeux,	Na	37	—15	22
Rigel,	H	30	—15	15
Castor.	H	40 to 45	—17	23 to 28
Regulus,	H	30 to 35	—18	12 to 17
β Ursæ majoris,	H	------	----	------
γ " "	H	------	----	------
δ " "	H	30	—9 to 13	17 to 21
ε " "	H	------	----	------
ζ " "	H	------	----	------
β Leonis,	H	------	----	------
δ "	H	------	----	------
η Ursæ majoris,	H	------	----	------
α Virginis,	H	------	----	------
α Coronæ borealis,	H	------	----	------
Procyon,	H	------	----	-----
Capella,	H	------	----	------
Aldebaran?	Mg	------	----	------
γ Cassiopeiæ,	H	------	----	------

* Dr. Huggins was anticipated in his observation of this *fourth line* by Lieut. J. Herschel and by Prof. J. Winlock, who independently discovered it; the first on the night of Oct. 25, 1868, at Bangalore, India, the second, with perfect distinctness, at Harvard Observatory on the night of Nov. 13, 1868. Mr. Huggins was informed as to the previous observation of Prof. Winlock, but does not mention it in his paper.—A. M. M.

TABLE II.—*Stars approaching the Sun.*

	Compared with	Apparent motion in miles.	Earth's Motion.	Motion toward Sun.
....	Mg	50	+5	55
....	H	40 to 50	+3·9	44 to 54
....	H	30	+9	39
....	Mg	32	+17	49
oris, ...	Mg	35 to 50	+11	46 to 60
....	Mg	......	....	......
....	Mg	......	....	......
....	H	......	....	......
....	H	......	....	......
....	H	......	....	......
da, ...	H	......	....	......

above determinations Dr. Huggins makes the following
—

velocities of approach and of recession which have been
to the stars in this paper represent the whole of the mo-
e line of sight which exists between them and the sun.
ow that the sun is moving in space, a certain part of
erved velocities must be due to solar motion. I have not
d to make this correction, because, though the direction
n's motion seems to be satisfactorily ascertained, any esti-
t can be made at present of the actual velocity with
is advancing must rest upon suppositions, more or less
, of the average distance of stars of different magnitudes.
not improbable that this part of the stars' motions may
than would result from Otto Struve's calculations, which
the supposition that the average parallax of a star of the
nitude is equal to 0″·209, a velocity but little greater
fourth of the earth's annual motion in its orbit.

ll be observed that, speaking generally, the stars which
roscope shows to be moving from the earth (Sirius, Betel-
gel, Procyon) are situated in a part of the heavens oppo-
ercules, toward which the sun is advancing, while the
the neighborhood of this region, as Arcturus, Vega,
show a motion of approach. There are in the stars
bserved exceptions to this general statement; and there
other considerations which appear to show that the sun's
space is not the only or even, in all cases, as it may be
e chief cause of the observed proper motions of the stars.
can be little doubt but that in the observed stellar
ts we have to do with two other independent motions—
movement common to certain groups of stars, and also
peculiar to each star.

roctor has brought to light strong evidence in favor of
of the stars in groups, having a community of motion,
aphical investigation of the proper motions of all the
e catalogues of Mr. Main and Mr. Stone. The proba-
all the stars being collected into systems was early sug-
Michell and the elder Herschel. One of the most
le instances pointed out by Mr. Proctor are the stars
, ζ, of the Great Bear, which have a community of

proper motions, while α and η of the same constellation have a proper motion in the opposite direction. Now, the spectroscopic observations show that the stars β, γ, δ, ϵ, ζ, have also a common motion of recession, while the star α is approaching the earth. The star η, indeed, appears to be moving from us, but it is too far from α to be regarded as a companion of that star.

"Although it was not to be expected that a concurrence would always be found between the proper motions which indicate the apparent motions at right angles to the line of sight and the radial motions as discovered by the spectroscope, still it is interesting to remark that in the case of the stars Castor and Pollux, one of which is approaching and the other receding, their proper motions are also different in direction and in amount; and further, that γ Leonis, which has an opposite radial motion to α and β of the same constellation, differs from these stars in the direction of its proper motion."—*Proc. R. S. of L.*, vol. xx, No. 136. A. M. M.

IV. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *National Academy of Sciences.*—The following is a list of papers read at the meeting of the National Academy of Sciences, held at Cambridge, Nov. 20, 1872:

1. The organization of the Museum of Comparative Zoölogy in Cambridge; by *L. Agassiz.*
2. On three different modes of teething among Selachians; by *L. Agassiz.*
3. On the manufacture of Gunpowder for great guns, and on increasing the efficiency of small arms by improved ammunition and sights; by *M. C. Meigs.*
4. An Acoustic Pyrometer; by *Alfred M. Mayer.*
5. Results of the Coast Survey astronomical expedition to the Rocky Mountains; by *Chas. A. Young.*
6. Presentation of an isothermal chart and of a hypsometric sketch of the United States; by *Chas. A. Schott.*
7. Account of the proceedings of the International Standards Commission at Paris, September, 1872; by *J. E. Hilgard.*
8. Development of Actiniae; by *Alex. Agassiz.*
9. The glacial phenomena of the Southern hemisphere compared with those of the North; by *L. Agassiz.*
10. Affinities of Echinoderms and Worms; by *A. Agassiz.*
11. On the construction and advantages of a large Aneroid barometer; by *M. C. Meigs.*
12. Notice of investigations making in California on the reliability of the barometer as a hypsometric instrument; by *J. D. Whitney.*
13. Pedicillariae of Echinoderms; by *A. Agassiz.*
14. On the determination of the relative intensities of sounds, and on the measurement of the powers of various substances to transmit and reflect sonorous vibrations; by *A. M. Mayer.*
15. Experimental Exhibition of the exploration of an Acoustic Wave Surface; by *A. M. Mayer.*
16. Researches on the change of dimensions of Iron and Steel rods and of hollow iron cylinders by their magnetization; by *A. M. Mayer.*
17. Analytical Notices; by *W. Gibbs.*
18. Results of recent dredgings on the coast of New England; by *A. E. Verrill.*
19. Tidal Researches; by *W. Ferrel.*
20. Embryological fragments concerning the Volutidae; by *L. Agassiz.*
21. On the specific identity of some animals along the Atlantic and Pacific [illegible] of America; by *L. Agassiz.*

[illegible] The copulatory organs of the Selachians compared with one another and [illegible] other Vertebrates; by *L. Agassiz.*

On the changes Selachians undergo with age; by *L. Agassiz.*
Critical remarks about scientific views entertained upon theoretical grounds; *Agassiz.*
Observations on the nature and duration of lightning; by *O. N. Rood.*
Notice of the progress of the topographical work of the Geological Survey ifornia; by *J. D. Whitney.*
The 1474 Corona line; by *C. A. Young.*
Mathematical reversal and semi-reversal; by *Benjamin Peirce.*

Discovery of Mastodon remains in Ohio; by L. E. HICKS, of Nat. Sci. (Communicated.)—Mr. C. W. Bryant, engineer e route of the Atlantic and Lake Erie R. R., has submitted e for examination a large mammalian bone. I find it to be eft side of the pubic arch of the pelvis of a *mastodon.* Di- ions as follows: Length, 29·5 centimeters; breadth at inner 16 centimeters; breadth at outer end 14·5 centimeters; thick- in center 9 centimeters. It was discovered in the bank of oon creek, near Granville. The fractured ends are much ed, and the whole specimen has the appearance of having rolled and worn by running water before it was deposited in natrix of clay and gravel. Portions of the matrix adhere y, and there is also a partial coating of iron oxide. The ific gravity is greater than that of fresh bone.

nville, O., Dec. 6, 1872.

Niagara: Its History and Geology, Incidents and Poetry, Illustrations; by GEO. W. HOLLEY. 165 pp. 12mo, with a : 1872. New York City. Sheldon & Co.—Mr. Holley has led for over thirty years at Niagara Falls, and in the little me he has lately given us, he records his own observations the curious facts of various sorts which he has collected it the great cataract. The chapters on the topography and ogy of this region are particularly interesting. From the eys of Capt. Williams for a canal around Niagara Falls and other sources, he compiles a comprehensive and clear state- t of the barrier which formed the eastern margin of Lake igan when that lake must have been the largest body of water on the globe, before there was a Niagara and when es Huron, St. Clair and Erie were only portions of the greater higan, whose waters form their outlet by the Illinois valley the Gulf of Mexico. These facts as well as the details of the gara section, its retrocession and surrounding phenomena, are liar to all students of American geology. But to the tourist general reader the volume of Mr. Holley, which is a free-will ing from an enthusiastic lover of natural scenery, and of this d passage in nature in particular, will be an acceptable addi- to a department of literature in which, as yet, so little has done in this country. The pleasure of the reader is often red by faults of style, ambitious efforts at fine writing, which neither to clearness or accuracy, which, with other minor ishes, a little judicious pruning would remove.

Photographs of the Hot Springs, Geysers and Scenery in the ion of the Yellowstone National Park, illustrating the Geological Survey of the Territories under the Department of the Interior.—Dr. Hayden's expedition, through the labors of Mr. W. H. Jackson, of Washington City, the photographer, has added to the photographs of 1871 an extended series of pictures of the wonderful objects seen during the past summer. Next to a personal visit to this land of geysers, hot springs, fountains of boiling mud, waterfalls, lakes and majestic mountains, is a morning spent over these photographs. They would do credit to the best photographic laboratory, and considering the difficulties inherent in a long and arduous journey, they are really admirable.

The Yellowstone series well illustrates the advantage of photography over any hand drawings in bringing out details of structure, especially where the artist is guided by the geologist in selecting the best points of view. Among the novelties which are a positive addition to our knowlege of orography we mention particularly the views of the Three Tetons. Among the Geyser views there are two of "Old Faithful" in full action, which are exceedingly effective; others of basins and cones in which the varied tracery of the surface may be studied with much of the satisfaction to be had from actual examination; others of long cascade slopes which have been gracefully terraced by the mineral depositions of the waters, and whose basins, brimfull to their delicate edges with the petrifying waters, reflect mirror-like the surrounding objects; others showing large areas of the Geyser region with the geysers in action. Such views give an opportunity for the geologist to compare beds of chemical deposition with our ordinary limestones.

There are already 600 of these views, and the Government has given permission to have them sold at moderate prices. There are three series of sizes, one 11×14 inches at $1 each, a medium size, 8×10 at 50 cts. each, and stereographs at $3 per dozen.

5. *A Popular Treatise on Gems;* by DR. L. FEUCHTWANGER. 528 pp. 12mo. New York, 1872 (author, No. 55 Cedar st., New York). With the exception of a brief addition to the Appendix, this is a reprint of the last edition of Feuchtwanger's valuable and well illustrated work on gems.

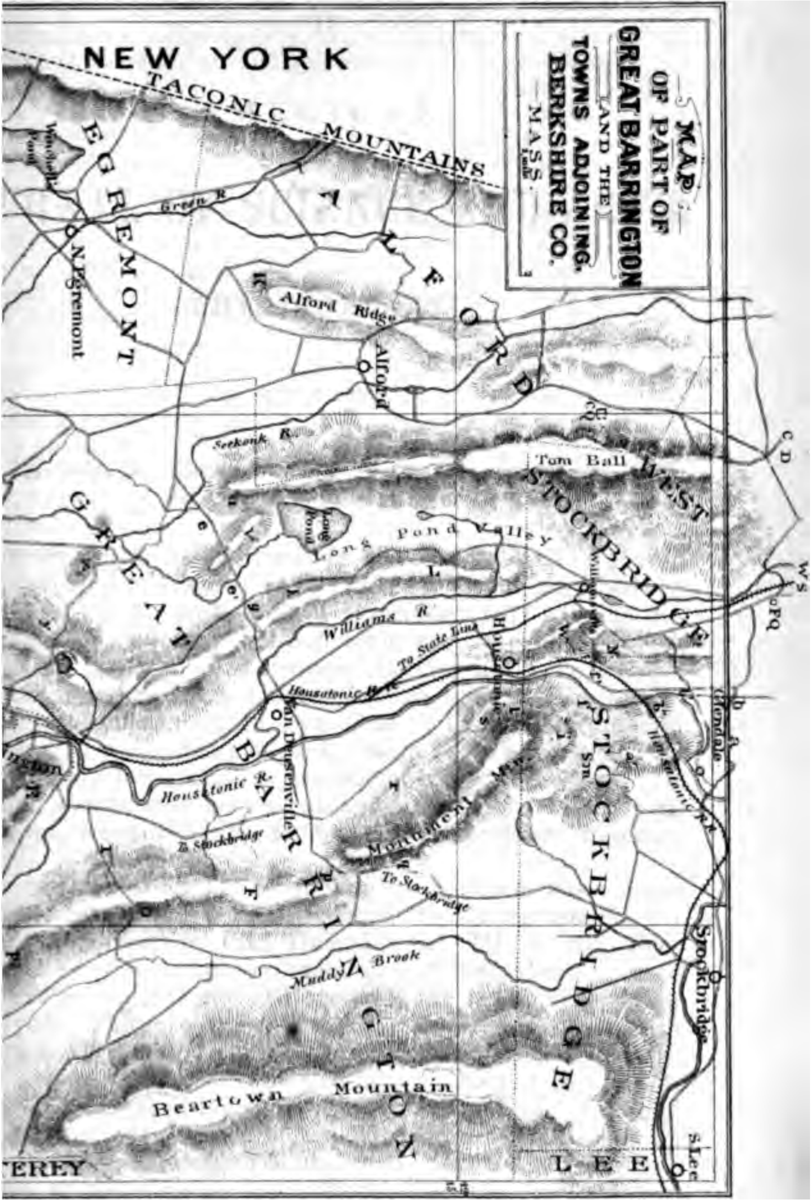
6. *A Manual of Microscopic Mounting, with notes on the collection and examination of objects;* by JOHN H. MARTIN, 200 pp. 8vo, with many illustrations drawn by the author. Philadelphia, 1872. (Lindsay & Blakiston). From the London edition.—This work is an exceedingly valuable companion to the student in microscopy. It describes apparatus, methods of mounting and preservation, and methods of examination, with all the fullness of detail required by the practical microscopist. The drawings are numerous and most of them original, and much in the work is new to science.

The Expressions of the Emotions in Man and Animals. By Charles Darwin. 374 pp. 12 mo. London, 1872 (John E. Murray).

Descriptive Catalogue of Minerals, being the Collection of William Nevill, F.G.S. Godalming, Surrey, 1872. pp. 159. London (printed by Taylor & Francis).

The Earth a Great Magnet; by A. M. Mayer, Ph. D., Prof. of Physics in the Stevens Technological Institute of Technology. 284 pp. 12mo, 1872. New Haven: C. C. Chatfield & Co.

I. VOL. V. PLATE 1. 1873.



THE

AMERICAN

JOURNAL OF SCIENCE AND ARTS.

[THIRD SERIES.]

ART. IX.—*On the Spectrum of the Aurora of October 14th*, 1872; by GEORGE F. BARKER.

ANOTHER very brilliant aurora was visible in New Haven on the evening of October 14th, 1872. Like the one observed the previous year—on the 9th of November, 1871—it was distinguished by its intense crimson color, and by its form—which was that of a single broad streamer shooting up in the western sky from near the horizon almost to the zenith. The brightness of this streamer varied from time to time in its different portions. It was accompanied by more or less white aurora, which latter, however, was considerably more diffused. It appeared shortly after six o'clock and lasted until nearly half-past seven, when it became too faint for spectroscopic measurement.

The instrument used in examining its spectrum was a single prism spectroscope of Duboscq, similar to the one used for the aurora of November, 1871 (this Journal, III, ii, 465, Dec., 1871). The prism is an equilateral one of flint, dense enough to distinctly separate the D lines with the magnifying power employed. The spectrum of the aurora, as seen in this instrument, was very bright and consisted of *seven* lines and bands, being markedly different from that of the aurora of Nov., 1871, the bands being crowded more together toward the middle of the spectrum. By means of a candle flame the divisions on the millimeter scale were illuminated, and the sodium line brought to the division 100. Even when the scale-numbers were distinctly visible, the auroral lines could be clearly and sharply distinguished. The divisions of the scale which cut the lines centrally were recorded, the slit being about a millimeter wide. Measured in this way, the seven auroral lines and bands, beginning at the least refrangible, had the following positions on the scale: 89·5, 110, 120–125, 132–135, 138–142, 150–155, and 181. The bands extended over the divisions of the scale above given. These numbers are the mean of three closely accordant and complete measurements by myself, and of one by my friend, Mr. C. B. Dudley, of this city. Counting the lines in order from red to blue, the brightness of the seven was as follows: 2, 1, 7, 3, 6, 4, 5; the line marked 110 being the brightest. The lines 89·5 and 110 were sharp on the edges, the line 181 nearly so, and the lines 3, 4, 5, and 6, from the red end, were more or less broad bands nebulous on the edges, but shading away equally apparently on each side. The measurements may be regarded as accurate to within half a division of the scale.

The value of these scale-numbers in wave-lengths was determined, as before, by a series of measurements of certain of the characteristic elemental lines and of the principal lines of Fraunhofer in full sunlight. The elemental lines measured had the following readings upon the scale of the instrument:

K α 66, Li α 81, Sr β 81·5, H α 83, Sr γ 84, Ca α 92, Li β 94, Sr α 95, Na α 100, Ca β 111·5, Tl α 119, H β 141·5, Sr δ 157, H γ 181, Ca γ 187, K β 206.

The scale-numbers for the Fraunhofer lines read as follows:

A 66, *a* 72·5, B 77, C 83, D 100, E 122, *b* 126, F 141·5, G 181.

The wave-lengths of these lines being taken from Angström's tables as given by Gibbs, the wave-lengths of the auroral lines were obtained by direct interpolation from these; it being assumed that no error greater than those of the instrumental measurement would be thus introduced. The following table gives the auroral data as obtained thus far:

Lines.	Scale number.	Wave-length.	Auroral lines.	Other measurements.
A	66	761		
a	72·5	716		
B	77	687		
C	83	656		
(1) line	89·5	630	630	630 N. R. Procter. 629·7 Vogel. 627·9 Zoellner. 628 Barker.
D	100	589		
(2) line	110	555	555	562 Barker. 557 Winlock. 556·9 Vogel. .. 556·7 Angström.
E	122	527		
(3) band	120–125	533–520	533–520	532 A. Clark, Jr. 531 Winlock. 523·3 Vogel. 530 Winlock.

ines.	Scale number.	Wave-length.	Auroral lines.	Other measurements.
b	126	517		
(4) band	132–135	505–499	505–499	502 Barker. 500·3 Vogel.
(5) band	138–142	493–485	493–485	485 A. Clark, Jr. 482 Barker.
F	141·5	486		
(6) band	150–155	474–467	474–467	469·4–462·9 Vogel. 464 Winlock.
G	181	431		
(7) line	181	431	431	434 A. Clark, Jr.

In this table, column 1 gives the lines observed, both Fraun-fer and auroral; column 2, the corresponding number on the ale of the instrument; column 3, the wave-lengths calcu-ted as above described; column 4, the auroral lines given in ave-lengths by themselves, for comparison; and column 5, e wave-lengths of apparently the same auroral lines, as meas-ed at other times or by different observers.

Of these seven lines it appears that, while none of them are w, yet that no previous observer has seen all of them at once, ogel having seen five, and four having been seen by myself a previous aurora. Two of these lines, the 5th and 7th, pear to coincide nearly with the solar lines F and G. But e want of a line corresponding to the C line, shows that these es cannot be due to hydrogen. Moreover, the 3d band cludes the E line within it. On plotting the spectrum of is and of the aurora of Nov., 1871, according to the scale-mbers given in the table, it is easy to see a considerable dif-rence between them. While four of the lines, 1, 2, 4 and 5, e common to both, three lines of the former have no corres-nding lines in the latter. Indeed the most refrangible line, wave-length 431, has only been once before observed and easured; this was by Alvan Clark, Jr., the reductions being ade by Professor Pickering. The existence of the band 3 tween 2 and 4, gives the spectrum its more condensed appear-ce, which was apparent on the very first glance through the ectroscope.

These results would seem to establish the necessity of as curate measurements as possible in fixing the position of roral lines. It is certainly clear that if the identity between ese lines and those of the air-spectrum—of course under mod-ed conditions—is to be established (as Professor Vogel thinks ill be the case), this can only be done by absolute verification lines by measurement. If the auroras at different times are different heights, which seems to be proved, then the elec-ic discharge must take place under varying conditions both temperature and pressure; conditions already abundantly own to have a marked effect on the spectrum. By a suffi-ent number of accurate line-measurements, therefore, it may be possible, not only to settle this question of identity with the air-spectrum, but also to get some approximate ideas upon the temperature and pressure in the auroral regions, and to determine the reasons of the differences observed in the spectra of various auroras. Pocket-spectroscope examinations may give a general idea of the spectrum, but they cannot serve for any exact determinations.*

New Haven, Dec. 30, 1872.

ART. X.—*On the Quartzite, Limestone and associated rocks of the vicinity of Great Barrington, Berkshire Co., Mass.*; by JAMES D. DANA.

[Continued from page 53.]

2. *From the Housatonic valley westward—continued.*

IN the ridge (L) to the southwestward of Housatonic village, the quartzite stratum, instead of being replaced by mica slate, as is the case to the north and northwest, is quartzite still; moreover, the underlying stratum of gneiss, s^1, *is quartzite also*, so that the limestone—the outcropping rock of Long Pond valley—is *directly overlaid by quartzite.* Further, these rocks, in place of being nearly horizontal in position, are nearly vertical.

Figure 6 represents an east and west section across this region a mile north of Vandeusenville, and extending west through the Tom Ball ridge into Alford.† (Fig. 3, of a section through Williamsville, and 5, of one through the north end of Tom Ball, are here repeated for comparison). The quartzite of the ridge L in this section is overlaid by gneissoid mica schist on the east, and has the limestone of Long Pond valley conformable to it on the west. The limestone is in many places in close contact with the quartzite, and the strike near the junction is N. 7°–20° E. with the dip mostly 70° to the eastward, but varying from 50° to 90°. The quartzite is intensely hard, showing, like that of Monument Mountain, no trace of bedding. For this reason an examination of the conıct of the limestone and quartzite tells us nothing as to 'hether the quartzite conforms to the limestone in dip, or 'hether the two are separated by a fault. But this question is ;ttled positively by *the existence of a laminated bed of quartzose 'mestone*, or calcareous quartzite, 40 to 50 feet thick, *in the uartzite itself*, as exhibited in the section (fig. 6). The lamina- .on of this calcareous stratum is very perfect and uniform, nd extends for a long distance along the west slope of the idge. Its strike is N. 5° E., and its dip 70°–75° to the east-

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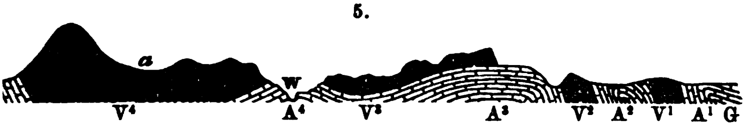
* An examination of the spectrum of this aurora with Hawkins and Wale's direct-vision pocket spectroscope showed that it coincided apparently with the spectrum seen in the larger instrument, except that the bands were not all separated from each other, owing to the less dispersion. Hence the five lines observed by Holden and described by him in the December number of this Journal. To my eye, however, the spectrum presented an appearance quite different from that figured by him. It would thus appear that the spectrum of the aurora, like the form of the solar corona, is liable to be affected by the personal equation of the observer.

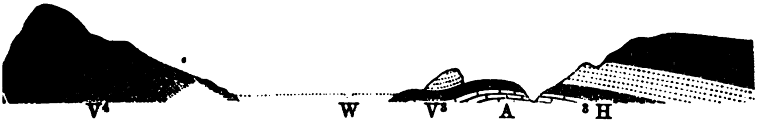
† Above V^4, in the section fig. 6, is the Tom Ball ridge; above A^4, the Long Pond valley; above L, the ridge L; at W, Williams river; at H, the Housatonic river, with low plains either side.

5.



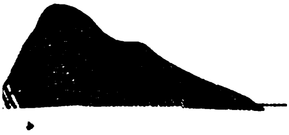
Section from Glendale westward, through north end of Tom Ball.

3.

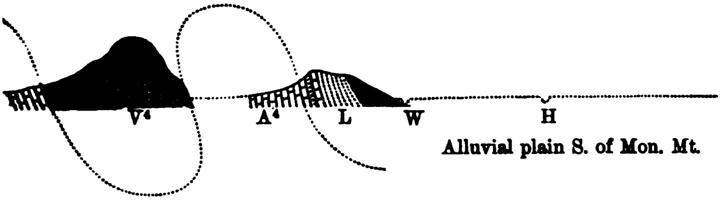


Section from Monument Mountain westward, through middle of Tom Ball.

3a.



6.



Section across Long Pond valley through southern half of Tom Ball.

'ard. The impure limestone or calcareous quartzite contains ccasionally minute, slender, brown tourmalines, some of them alf an inch long. The stratum is separated from the limestone n the west by about forty feet of the hard bedless quartzite, ıe part of the quartzite that corresponds in thickness with the ›wer gneiss in the Monument Mountain section (figs. 3 or 4) n the Housatonic valley. To the east of this calcareous stra- ım there is a great thickness of quartzite, 200 feet or more; ıe exact amount cannot be determined on account of the bsence of bedding.

To the east of the quartzite the slopes (the eastern of ridge L) are covered with earth, showing that there is a soft, decomposable rock beneath, probably mica schist; and toward the foot of the slopes there is an outcrop of the mica schist, dipping 28° to the eastward, the strike being north. This amount of dip is very much less than that of the western side of the quartzite; and it is probable that the dip in the quartzite gradually diminishes to the eastward. The mica schist is evidently the stratum s^2 of Monument mountain, while the quartzite corresponds to q^1 and s^1 combined. The limestone of Lond Pond valley is l^1; while that in the quartzite is a layer not before noted, which we may call l^2.

The slate of the part of Tom Ball in this section is the smoothish mica slate, like that to the north, but it differs in carrying the high dip of 60°–70°, even down to its eastern foot. The strike is north or nearly so, like that of the limestone and quartzite of the opposite side of Long Pond valley. On the western slope of the Tom Ball ridge the rocks are mostly concealed by earth; but there are many exposures of limestone in Alford over the plain at its foot, and in these the beds have an average strike of N. 5°–10° E., and a dip of 50°–70° to the eastward; in some places 90°.

b. Continuing this section westward across the town of Alford, we pass from the limestone of eastern Alford to the mica slate of Alford ridge. The slate and limestone at their junction have the same strike and dip; the strike observed (just east of K on the map) being N. 7° E. and the dip 70° to the eastward.

In the *western* Alford valley there is again limestone with a high dip, and beyond this the mica slate of the Taconic ridge. The most western outcrop of limestone observed, or that nearest to the Taconic ridge, gave for the strike N. 19° E. and dip 52°; and the same was obtained as the average for the slate of the ridge, a hundred yards distant. The limestone is evidently conformable to both the slate of Alford ridge and the Taconic. This slate in each is identical in its characters with that of the Tom Ball ridge—a smooth-surfaced mica slate, partly chloritic and more or less garnetiferous, and containing many quartz veins the cavities of which are often filled with chlorite.

c. We come now to the question as to the folds along this section; and, in connection, the character of the fold along the Tom Ball ridge elsewhere.

At the north end of the Tom Ball ridge the existence of a synclinal is fully demonstrated, as shown in fig. 5. The limestone strata at the eastern and western foot here dip toward each other beneath the slates of the ridge; and moreover the limestone emerging on the east is directly continuous around the north end of the mountain with that on the west. It remains, therefore, only to trace out the changes in the synclinal fold to the southward through the rest of the mountain.

Along the section in fig. 6.—The low quartzite ridges W and L (see map) are overlapping parts of an interrupted series, in which L is situated half a mile to the west of the line of W. The high inclination of the strata in L is evidently connected with this more western shove of the ridge. In consequence of it the limestone is bent up into a close fold and the Tom Ball ridge west of it into another equally close, the strata having a dip of from 50° to 75°; and as the slates of Tom Ball form a synclinal, the limestone corresponds to an anticlinal. This is indicated in the curved lines in fig. 6.

I may add that in the limestone of eastern and western Alford there are probably other anticlinals. With regard to these more western ranges of limestone, especially that west of Alford ridge, the evidence is not yet complete.

Along the section in fig. 3.—To understand the fold abreast of Williamsville we must note that in this section the limestone of the Housatonic fold, A^3, dips westward, and continues apparently at no great distance beneath the surface to the Tom Ball ridge, where the foot rocks on the east are nearly horizontal; and then, after passing under the mountain, emerges on the west at a high angle like that of the Tom Ball slates adjoining. Now, following the strata from Monument Mountain westward, it is plain that they do not dip beneath the surface in the Williamsville valley, and thence bend up into Tom Ball; but their course, as just stated, is nearly horizontal till reaching the present position of Tom Ball. The downward bend in the synclinal, therefore, took place along what is now the eastern slope of the mountain. In fig. 3 the part of the section along the junction of the horizontal and steeply-dipping slates is left blank; fig. 3 A shows the same with this bend in the layers.

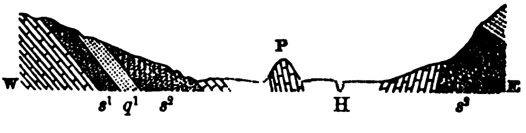
In section 6, the quartzite of the ridge on the east (L) is unrepresented on the opposite or western side of Long Pond valley. It ought to appear, if existing there, between the slates of the ridge and the limestone of the valley. As there is no outcropping limestone very near the slates, its actual absence is not certain. Directly west of Long Pond the bluff eastern front of the Tom Ball ridge bends southwestward, and there is a ridge between it and the lake, two hundred feet or so high, which has no rock at its surface and whose composition I could not ascertain. Then, west of the south end of Long Pond, the ridge which makes properly the southern end of the Tom Ball line is a region of outcropping quartzite of the hard, jointed, nonbedded kind. This quartzite region is marked *u* on the map.

Another ridge of quartzite of similar character starts near the same point and stretches southeastward, crossing the road from Vandeusenville to Alford just west of Long Pond brook. It is marked *v* on the map. No section was found showing the relation of the Tom Ball slates to the quartzite: but the limestone of Long Pond valley, east of the quartzite ridge (at *e′*) and that on the west of it (near *e*, either side of the road) both dip toward the ridge, the latter at an angle of 25° to 30°; and thus it is proved that the quartzite is a stratum directly overlying the limestone, and, therefore, the same that exists in ridge W. It hence follows that the beds of Tom Ball, while all mica slate at the north end, are replaced by quartzite in their bottom portion at the south end of the ridge. From *e*, limestone is continuous westward and then northward into and through Alford, and also southward over Egremont; while from *e′* the limestone extends eastward to Vandeusenville; and in this part it is various in its strike and dip. Near *g* the dip is to the southwestward, being at the more southern outcrop 35°, with the strike N. 50° W.; then, a few rods to the north, 40°, strike N. 35° W.; then 50° to 70°, strike N. 35° to 24° W.; then farther north, near the quartzite, 70° to the eastward, strike N. 5° to 10° E. This range of outcropping limestone, extending east to Vandeusenville, stops off the quartzite ridge L; or, in other words, the quartzite, which is the overlying rock, does not extend across it. Along the road opposite the iron furnace, just west of Vandeusenville, the bedding of the limestone is obscure; but the strike appears to be east and west and the dip northward 50° to 60°.

5. I pass now to the fifth of the western sections, or that in the line of Great Barrington, two miles south of Vandeusenville.

In this section, fig. 7, the limestone on the left (west) is that of the Egremont region, already shown to be identical with that of Alford, Glendale and Stockbridge. It dips under three hundred feet or more of schist (mica schist and gneiss), in which, as the section shows, there is a bed of quartzite. The hill rises directly from the railroad track at Great Barrington, and has the limestone outcropping at its highest part, near *z* (map), as well as along the lower of its *western* slopes.

7.



Section across the Housatonic valley through Great Barrington.

The dip of the limestone in this section is mostly between 5° and 55°; the mean strike is N. 10° E. The dip of the chist is, with small exceptions, 35° to 40°, and the strike N. 0° to 20° E. There is a wrench in the ridge south of the ighest part (z on the map), so that the strike varies; being N. 10° to 20° W., at points northeast of z; N. to N. 10° E., o the east of it, or at the marble quarry; then, N. 25° E., and finally N. 40° to 50° E., 150 yards to the east of south; and this last strike is found across this part of the ridge at the western foot.

The evidence of the existence of a bed of quartzite in the idge is small but positive. Hard-jointed quartzite outcrops at a point toward the upper limit of the schist, S. E. of z, for a distance of 12 or 15 yards, and also at a second point above, both of them west of the village. The strike of the outcrop is N. 0° E., conforming to that of the limestone above it, its position being in the wrenched portion of the ridge. The thickness of he bed may not be more than fifteen yards, as the outcrop is no wider; but the shortness of the outcrop in the line of the bedding is proof that the bed is mainly the soft quartzite; and f so, it may be 100 feet or more in breadth. Three-quarters of a mile to the north, along by y, near a road crossing the ridge, the surface of the fields is thickly strewn with great blocks of quartzite, which seem to indicate that the bed exists beneath, and has considerable width. This range of quartzite masses continues near the road to the eastern of the spurs of quartzte, at the south end of Tom Ball ridge; and at x there is a ow hill of outcropping quartzite. These facts connect the rocks of the Tom Ball and Long Pond region with those of the idge just west of Great Barrington, giving positive proof that the quartzite is the lower quartzite, or q^1.

The ridge west of Great Barrington consists, consequently, above the underlying limestone, of (1) a lower stratum of schist s^1); (2) a bed of quartzite (q^1); (3) an upper bed of schist (s^2) much thicker than the lower (s^1) There is no upper quartzite.

This section introduces a new element, *an upper stratum of limestone*, overlying the upper schist (s^2), where the upper quartzite would be looked for. It outcrops in the valley near the railroad and also east of the river and village. The limestone is a bluish-gray and firm granular variety. Some portions are quite pyritiferous; and at one spot (near the Maple avenue crossing) I found minute brown tourmalines with the pyrite. Just west of the railroad track, 60 yards north of the Maple avenue crossing, the limestone outcrops within fifteen yards of the schist, and both have the strike N. 7° to 10° E., and eastward dip 65°. There is another outcrop at the Maple avenue crossing, giving the strike N. 8° E. and dip 40° to 35°.

More to the eastward the dip increases, it being in the ledge called Mt. Peter (P, fig. 7), east of the principal street of the village, 70° to 80° to the eastward, and mostly obscure; and again, east of the river, toward East Mountain (a third of a mile east of the Berkshire House), 80° to 85° *to the westward*, with the strike nearly north, or between N. 10° E. and N. 10° W. I have been unable to find evidence that this limestone is a continuation, in a fold, of that of Egremont.

This Great Barrington section (fig. 7) terminates eastward in the slopes of East Mountain, in which the rock, a durable gneiss through the lower half with 120 feet of quartzite above, dips 60° to 50° in the outcrops nearest the limestone, diminishing eastward to 50° and 40°, with the strike about N. 10° E. Some of the outcrops of limestone and gneiss are not over ten yards apart. The unconformability between the gneiss and limestone is evidence of a fault along a fracture. The gneiss is for the most part finely contorted in its grain, and in some portions coarsely so. The quartzite overlying it is stratified, not very firm, and slightly gneissoid in places. It will be shown beyond that the gneiss is probably a repetition of that to the west of Great Barrington, that is, either s^2 of Monument Mountain, or this stratum along with the others beneath it. The pressure that attended the faulting and uplift accounts for the limestone here standing on end and being even reversed a little in dip adjoining the East Mountain gneiss; and also for the contortion of the gneiss.

The following conclusions flow from the facts stated.

X. The lower quartzite and gneiss vary in mineral constitution south of the Williamsville section, fig. 3, as they do to the north; the facts need not be repeated. The stratum of mica schist and gneiss, s^2, in Monument Mt., which becomes mica slate in the northwest margin of the mountain and the ridges west, including Tom Ball, is mica schist and gneiss again in the ridge west of Great Barrington; and in that east of Great Barrington it is a firm gneiss, breaking into huge blocks—many such covering parts of the slopes. These differences are due partly (*a*) to original differences in mineral composition; but partly (*b*), in all probability, to differences in the conditions attending metamorphism, such as the amount of heat, the amount of moisture present, the amount of pressure and of resistance to the pressure. None of the smooth mica slate, like that of Tom Ball and the Taconic Mountains, occurs in this part of Berkshire east of Great Barrington.

XI. The synclinal fold in the Tom Ball ridge is at its southern end close-compressed between the limestone anticlinals, and dwindles out in that direction; while, at the northern end, it is broadly expanded and the limestone emerges from beneath it, the eastern and northeastern portion at a small angle.

XII. The conformability of the western range of limestone in Alford with the slates of the Taconic ridge still farther west has, in an early part of this memoir (p. 2), been made a basis for the conclusion—the ordinary one of geological writers on the subject—that *the Taconic slates are older than the Stockbridge limestone.* But it must be shown that the Taconic ridges are not the courses of synclinal folds, before this can be accepted as an *established* fact.

We may now turn to the region east of the Housatonic river.

(To be continued.)

ART. XI.—*Researches in Actino-chemistry.* MEMOIR SECOND. *On the Distribution of Chemical Force in the Spectrum;* by JOHN WILLIAM DRAPER, M.D., LLD., President of the Faculties of Science and Medicine in the University of New York.

[Continued from page 38.]

2d.—*Of the union of chlorine and hydrogen.*

An interesting experiment illustrating the fact that chlorine gas absorbs the radiations which bring about its combination with hydrogen, may be made by covering a test tube containing an explosive mixture of equal volumes of those gases with a large jar filled with chlorine. This arrangement may be exposed in the open daylight without risk of exploding the mixture, but if the experiment be made with a covering jar containing atmospheric air instead of chlorine, the gases immediately unite, and commonly with an explosion.

I placed a mixture of equal volumes of chlorine and hydrogen in a vessel made of plate glass, the edges of the pieces being cemented together. This vessel was so arranged on a small porcelain trough, containing a saturated solution of common salt, that it could be used as a gas jar. The radiations of a lamp were caused to pass through it, so as to be submitted to the selective absorption of the mixture. They were then received on a chlor-hydrogen actinometer.

Successive experiments were then made, 1st, with the radiations of a lamp after passing through the absorption vessel. 2d, with the same radiations after the vessel had been removed.

Two facts were now apparent, 1st, the mixture of chlorine and hydrogen in the absorption vessel began to unite under the influence of the rays of the lamp. 2d, the rays which had passed through that mixture had lost very much of their chemical force. It was not totally extinct, but the actinometer showed that it had undergone a very great diminution.

From this it follows that on its passage through a mixture of chlorine and hydrogen, the radiation had suffered absorption, and as respects the mixture under trial had become de-actinized, Simultaneously the mixture itself had been affected, its constituent gases uniting. And thus it appears that the radiation had undergone a change in producing a change in the ponderable matter.

The following modification of this experiment shows the part played by the chlorine and hydrogen respectively, when they are in the act of uniting.

(*a*) The glass absorption vessel above described was filled with atmospheric air, and the chemical force of the radiation passing from the lamp through it was determined. It was measured by the time required to cause the index of the actinometer to descend through one division. This was 12 seconds.

(*b*) The absorption vessel was now half filled with chlorine, obtained from hydrochloric acid and peroxide of manganese. The chemical force of the ray after passing through it was determined as before. It was now represented by 25½ seconds.

(*c*) To the chlorine an equal volume of hydrogen was added, the absorption vessel being consequently full of the mixture. The radiation was now passing through a stratum of chlorine diluted with hydrogen, and the point to be determined was whether it had undergone the same, or a greater, or less loss than in the preceding case, since the chlorine was now uniting with the hydrogen. On measuring the force it was found to be represented by 19 seconds.

(*d*) Lastly, the first (*a*) of these measures was repeated with a view of ascertaining whether the intensity of the lamp had changed. It gave 12 seconds as before.

From these observations it may be concluded that the addition of hydrogen to chlorine does not increase its absorptive power. Moreover, it is obvious that the action of the radiation is expended primarily on the chlorine, giving it a disposition to unite with the hydrogen, and that the functions discharged by the chlorine and by the hydrogen respectively are altogether different. The ray itself also undergoes a change; it suffers absorption and loss of a part of its *vis viva*.

As to the ray which is thus absorbed. In 1835 I found that a radiation which had passed through a solution of potassium bichromate failed to accomplish the union of chlorine and hydrogen; but one which had passed through ammonia sulphate of copper could do it energetically. This indicates that the effective rays are among the more refrangible. On exposing these gases in the spectrum, the maximum action takes place in the indigo rays (Phil. Mag., Dec., 1843).

Recently (1871) some suggestions have been made by M. Budde respecting the action of light upon chlorine. Admitting the correctness of the theorem, that the molecules of most elementary gases consist of two atoms, he conceives that the effect of light on chlorine is to tend to divide, or actually to divide, its molecule into isolated atoms. These atoms, if the gas be kept in the dark, may reunite into molecules.

The chlorine molecule cannot unite with hydrogen; the chlorine atom can; hence insolation brings on combination. But if the chlorine be unmixed, there will as a consequence of insolation be a certain proportion of uncombined atoms, and from this, together with Avogadro's theorem, is drawn the conclusion that this gas through insolation increases in specific volume. Moreover, as the reunion of the chlorine atoms probably produces heat, rays of high refrangibility will cause chlorine to expand, but it will contract to its original volume when no longer under the influence of light.

In corroboration of this conclusion Budde found that a differential thermometer filled with chlorine showed a certain expansion when placed in the red or yellow rays, but it gave an expansion six or seven times greater when in the violet rays. With carbonic acid and ether no such effect took place.

It should not be forgotten, however, in considering the bearing of these experiments, that chlorine merely because it is yellowish-green will absorb rays of a complementary, that is, of an indigo and violet color, and become heated thereby.

It has next to be determined whether the points of maximum action, that is, the points of maximum absorption, correspond to the rays of emission of either or both these gases, as they apparently ought to do under Angström's law. "A gas when luminous emits rays of light of the same refrangibility as those which it has the power to absorb."

Of the four rays characteristic of hydrogen there is one the wave-length of which is 4340. It is in the indigo space.

Plücker gives for chlorine a ray nearly answering to this. Its wave-length is 4338, and also another 4346, the latter being one of the best marked of the chlorine lines.

There are, therefore, rays in the indigo which are absorbed both by hydrogen and by chlorine. The place of these rays in the spectrum corresponds to that in which the gases unite—the place of maximum action for their mixture.

But the absorptive action of chlorine is not limited to a few isolated lines. The gas removes a very large portion of the spectrum. Subsequent experiments must determine whether each of these lines of absorption is also a line of maximum chemical action.

The chlor-hydrogen actinometer, referred to in previous paragraphs as depending for its indications on the union of chlorine and hydrogen, furnishes the means of ascertaining many facts respecting the combination of those substances advantageously, since it gives accurate quantitative measures.

By referring to my papers in the Philosophical Magazine (Dec., 1843, July, 1844, Nov., 1845, Nov., 1857) it will be found that chlorine and hydrogen do not unite in the dark at any ordinary temperature or in any length of time; but if exposed to a feeble radiation such as that of a lamp they are strongly affected. The phenomena present two phases: 1st, for a brief period there is no recognizable chemical effect, a preliminary actinization, or as Professors Bunsen and Roscoe subsequently termed it, photo-chemical induction, taking place. It is manifested by an expansion and contraction of the mixture. 2d, the combination of the gases begins, it steadily increases, and soon acquires uniformity. In obtaining measures by the use of these gases we must, therefore, wait until this preliminary actinization is completed. That accomplished, the hydrochloric acid arising from the union of the gases is absorbed so quickly, that the movements of the index-liquid over the graduated scale give trustworthy indications.

As regards the duration of the effect produced on the gases by this preliminary actinization, I found that it continued some time—several hours (Phil. Mag., July, 1844). Professors Bunsen and Roscoe, however, in their memoir read before the Royal Society, state that it is quite transient (Transactions R. Soc., 1856).

This preliminary actinization completed, the quantity of hydrochloric acid produced measures the quantity of the acting radiation. This I proved by using a gas flame of standard height, and a measuring lens consisting of a double convex, five inches in diameter, sectors of which could be uncovered by the rotation of pasteboard screens upon its center, the quantity of hydrochloric acid produced in a given time being proportional to the area of the sector uncovered. The same was also proved by using a standard flame, and exposing the gases during different periods of time. The quantity of hydrochloric acid produced is proportional to the time.

The following experiment illustrates the phenomena arising during the actinization of a mixture of chlorine and hydrogen, and substantiates several of the foregoing statements.

The diverging rays of a lamp were made parallel by a suitable combination of convex lenses. In the resulting beam a chlor-hydrogen actinometer was placed, there being in front of it a metallic screen, so arranged that it could be easily removed or replaced, and thus permit the rays of the lamp to fall on the actinometer or intercept them.

On removing the screen and allowing the rays to fall on the sensitive mixture in the actinometer, an expansion amounting half a degree was observed. In 60 seconds this expansion ased.

The volume of the mixture now remained stationary, no parent change going on in it. At length, after the close of 0 seconds, it was beginning to contract, and hydrochloric id to form.

At the end of 45 seconds more a contraction of half a degree d occurred; the volume of the mixture was, therefore, now e same as when the experiment began, this half degree of ntraction compensating for the half degree of expansion.

The rate of contraction of the gaseous mixture, that is, the te at which its constituents were uniting, was then ascertained.

From these observations it appeared that when chlorine and ydrogen unite, under the influence of a radiation, there are ur distinct periods of action.

1st. For a brief period the mixture expands.

2d. For a much longer period it then remains stationary in olume, though still absorbing rays.

3d. Contraction arising from the production of hydrochloric id begins; at first it goes on slowly, then more and more pidly.

4th. After that contraction is fully established, it proceeds ith uniformity, equal quantities of hydrochloric acid being roduced in equal times by the action of equal quantities of the ys.

The prominent phenomena exhibited by a mixture of chlore and hydrogen are a preliminary absorption and a subseuent definite action.

It may be remarked, since a similar preliminary absorption ccurs in the case of other sensitive substances; that there is in ractical photography an advantage, both as respects time and rrectness in light and shadow, gained by submitting a sensive surface to a brief exposure in a dim light, so as to pass it rough its preliminary stage.

The expansion referred to as taking place during the first of ese periods, may be advantageously observed when the disrbing radiation is very intense. It is well seen when a Leyen jar is discharged in the vicinity of the actinometer. Though is light lasts but a very small fraction of a second, it produces n instantaneous expansion, followed by an instantaneous conraction. Not unfrequently the gases unite with an explosion. have had several of these instruments destroyed in that anner.

It might be supposed that this instantaneous expansion is ue to a heat disturbance arising from the absorption of rays hat are not engaged in producing the chemical effect. But his interpretation seems to be incompatible with the instantaeously following contraction. Though it is admissible that heat should be instantaneously disengaged by the preliminary actinization, it is difficult to conceive how it can so instantaneously disappear.

When the radiation is withdrawn, and the hydrochloric acid absorbed, there is no after-combining. The action is perfectly definite. For a given amount of chemical action, an equivalent quantity of the radiation is absorbed.

The instances I have cited in this discussion of the mode of action of radiations are, one of decomposition, in the case of the silver iodide, and one of combination, in the case of hydrochloric acid. I might have introduced another, the dissociation of ferric oxalate, which I have closely studied, but it would have made the memoir of undue length. From the facts herein considered the following deductions may be drawn.

When a radiation impinges on a material substance it imparts to that substance more or less of its *vis viva*, and therefore undergoes a change itself. The substance also is disturbed. Its physical and chemical properties determine the resulting phenomena.

(1st.) If the substance be black and undecomposable, the radiation establishes vibrations among the molecules it encounters. We interpret these vibrations as radiant heat. The molecules of the medium do not lose the *vis viva* they have acquired at once, since they are of greater density than the ether. Each becomes a center of agitation, and heat-radiation and conduction in all directions are the result. The undulations thus set up are commonly of longer waves, and as the movements gradually decline the shorter waves of these are the first to be extinguished, the longer ones the last. This, therefore, is in accordance with what I found to be the case in the gradual warming of a solid body; in which the long waves pertain to a low temperature, the short ones arising as the temperature ascends (Phil. Mag., May, 1847).

In some cases, however, instead of the disturbing undulation giving rise to longer waves, it produces shorter ones, as is shown when a platinum wire is put into a hydrogen flame, or by Tyndall's experiments, in which invisible undulations below the red give rise to the ignition of platinum.

(2d.) If the substance be colored and undecomposable, it will extinguish rays complementary to its own tint. The temperature will rise correspondingly.

(3d.) If the substance be decomposable, those portions of the radiation presented to it which are of a complementary tint will be extinguished. The force thus disappearing will not be expended in establishing vibrations in the arresting particles, but in breaking down the union of those which have arrested hem, from associated particles. No vibrations therefore are riginated, no heat is produced, there is no lateral conduction.

In actinic decompositions the effects may be conveniently ivided into two phases; 1st, physical; 2d, chemical. The hysical phase precedes the chemical. It consists in a prelimnary disturbance of the group of molecules about to be decomosed. Up to a certain point the dislocation taking place may e retraced or reduced, and things brought back to their orignal condition. But that point once gained, decomposition enues, and the result is permanent.

I may perhaps illustrate this by a familiar example. If a heet of paper be held before a fire, its surface will gradually warm, and if the exposure be not too long, or the fire too hot, n removing it, the paper will gradually cool, recovering its ormer condition without any permanent change. One could onceive that the laws of absorption and radiation might not nly be studied but again and again illustrated by the exposure nd removal of such a sheet. But a certain point of temperaure or exposure gained, the paper scorches, that is, undergoes hemical change, and then there is no restoration, no recovery f its original condition. Hence it may be said of such a sheet f paper that it exhibits two phases, in the first of which a reurn to the original condition is possible, in the second such a eturn is impossible, because of the supervening of the chemcal change.

An investigation of the effects produced by a ray presents hen, these two separate and distinct phases, the physical and he chemical.

General Conclusions.

The facts presented in the former and the present memoir uggest the following conclusions:

1st. That the concentration of heat heretofore observed in the ess refrangible portion of the prismatic spectrum, arises from he special action of the prism, and would not be perceived in a iffraction spectrum.

2d. From the long observed and unquestionable fact, that here is in the prismatic spectrum a gradual diminution in the eat-measures from a maximum below the red to a minimum n the violet, coupled with the fact now presented by me, that he heat of the upper half of the spectrum is equal to that of he lower half, it follows that the true distribution of heat hroughout the spaces of the spectrum is equal. In conseuence of the equal velocity of ether-waves, they will on comlete extinction by a receiving surface generate equal quantiies of heat, no matter what their length may be. Provided, that their extinction takes place without producing any chemical effect.

3d. That it is incorrect to restrict to the upper portion of the spectrum the property of producing chemical changes. Such changes may be produced by waves of any refrangibility.

4th. That every chemical effect observed in the spectrum is in consequence of the absorption of a specific radiation, the absorbed or acting radiation being determined by the properties of the substance undergoing change.

5th. That the figure so generally employed in works on actino-chemistry to indicate the distribution of heat, light and actinism in the spectrum, serves only to mislead. The heat curve is determined by the action of the prism, not by the properties of calorific radiations, the actinic curve does not represent any special peculiarities of the spectrum, but the habitudes of certain compounds of silver.

ART. XII.—*Brief Contributions to Zoölogy, from the Museum of Yale College.* No. XXIV.—*Results of Recent Dredging Expeditions on the Coast of New England;* by A. E. VERRILL.

(Continued from page 16.)

THE Annelids obtained by Dr. Packard, both in the 150- and 110-fathom localities, were numerous and interesting, many of them being previously unknown in our waters. The number of species from these two localities was at least 55, of which 23 were common to the two localities, and were mostly found also in 85 fathoms (p). From the last named locality five additional species were also obtained. From 150 fathoms (s) there were 33 species, more than half of them not before recorded from America. Among the more interesting were (t, x) *Hermione hystrix* (?); *Lumbriconereis fragilis*; *Nothria conchylega* Malm.;[*] (o) *N. opalina* V. (new species, see page 102); +(o) *Goniada maculata* Œrsted; *Trophonia aspera* (Stimp. sp.); +*Scalibregma inflatum* Rathke; +(o) *Scolecolepis cirrata* Malmg.; +(o)*Pista cristata* Malmg.; (x) *Amage auricula* Malmg.; (x, o) *Melinna cristata* Mal.; (x, t) *Samythella elongata* V.[†] (a new genus

[*] The name "Nothria" was substituted for *Northia* (Johns.) by Malmgren for reasons that are scarcely sufficient. The latter name was, however, previously in use for a genus of shells (Gray, 1847), and must be rejected on that account.

[†] *Samythella*, gen. nov.

Body elongated, composed of about 50 segments, 15 of which bear fascicles of setæ; and posteriorly about 35 bear uncini only, but have a small conical papilla above the uncigerous lobe, as in *Melinna*; the uncini commence on the 4th setigerous ring. Branchiæ 6, placed side by side in a continuous transverse row. Cephalic lobe oblique, somewhat shield-shape, with a narrowed prominent front. Buccal lobe shorter. Tentacles numerous, smooth and slender.

nd species); (x, t) *Terebellides Stroemi* Sars; (o, p) *Maldane Sarsii* Malmg., which forms tough, parchment-like tubes and ements to them a great quantity of mud or fine sand, thus orming tubes or oval masses, often nearly an inch in diameter, lthough the worm itself may be less than one line. There ere also two species of Sipunculoids; one of which (*Phasco-osoma cæmentarium* (Quatr. sp.),* inhabits dead shells of vari-us Gasteropods, partially closing up the aperture with firmly emented mud and sand; this species is common also in the hallower waters of New England, from Martha's Vineyard orthward. The second species, *P. tubicola* V. (new species)†

This genus is closely allied to *Samytha* of Malmgren, in the structure of the ead and number of branchiæ, but differs in having a much larger number of egments (in this respect approaching *Melinna*), and in having only 15 setigerous egments, instead of 17.

Samythella elongata, sp. nov.

Body slender, composed of 54 segments in the specimens examined, tapering regu-arly to the posterior end. Cephalic lobe about as broad as long, broadly rounded osteriorly, with the postero-lateral corners prominent and well rounded, the ides slightly incurved and rapidly narrowing to the front, which is about half the idth of the back, and subtruncate, projecting forward; the middle region is a aised and convex oblong area as wide as the front edge, into which it runs. Buccal lobe a little shorter. Tentacles numerous, slender, tapering. Branchiæ ubequal, slender, tapering, about twice the length of the cephalic lobe. Setæ umerous and long in all the fascicles except the first three, the longest nearly one-hird of the diameter of the body. The posterior end of the body is surrounded y about eight small papillæ, of which the two upper ones are largest.

Length of the largest specimens, in alcohol, 1·5; diameter, ·10 to ·12 of an inch. he tubes consist of a thin and tough lining, to which a close layer of sand, in rains of moderate and nearly uniform size, is firmly cemented.

* Histoire Nat. des Annelés, Vol. II, p. 628, 1866. This is the *Sipunculus Bernhardus* of American writers, but not of Forbes. *P. hamulatum* Packard, Mem. Bost. Soc., II, p. 290, 1867, may perhaps be the same species.

† *Phascolosoma tubicola*, sp. nov.

Body versatile in form; in contraction short, cylindrical, oval, or fusiform, ·5 to 1 inch long, ·10 to ·15 in diameter; in full extension the body is more or less fusiform, gradually tapering anteriorly into the long, slender, nearly cylindrical retractile portion, which is longer than the body and bears, near the end, a circle of about 10 to 16, simple, slender tentacles, beyond which the terminal portion is often extended into a short proboscis, with the mouth at the end; below the tentacles there is sometimes a dilation, but this is without special spines or granules, and like the rest of the retractile portion in texture. The posterior end of the body is bluntly rounded, and the skin is transversely wrinkled and rough, and covered with small, round, somewhat raised verrucæ or suckers, to which dirt adheres, and at the end nearly always bears from 3 to 8, small, but prominent, peculiar bodies, having a slender pedicel and a clavate or globular head; their nature is doubtful (they may be sense-organs, but should be examined on living specimens); at about the posterior third of the proper body is an irregular zone of numerous, dark brown, hard, chitinous hooks, arranged in several rows, broad triangular in form, with acute points directed forward; among the hooks are also a few suckers; the middle region is covered with small, round, slightly raised suckers, which become much more prominent and crowded at the anterior end toward the base of the retractile portion, and have here the form of small, subconical, elevated warts, to which dirt usually adheres firmly; the retractile portion is covered throughout with minute conical verrucæ or papillæ, most prominent toward the base.

In many respects *P. cæmentarium* agrees very closely with this, but it has the posterior end much smoother, and with less conspicuous suckers; the hooks are forms a very coarse, short, thick tube, composed of mud and coarse sand firmly cemented together. This was an abundant species, both in this and the other muddy localities (o and p), but has not been found in shallow water. There were also species of *Nephthys*, *Amphicteis*, *Ampharete*, *Clymene*, *Sabella*, etc. Many empty fragments of thin, calcareous, nearly straight, round tubes, but having occasional swellings, occurred both here and in 110 fathoms; these belong, perhaps, to *Protula arctica* Sars.

The fauna of the locality in 110 fathoms (o), N. lat. 42° 5′, W. long. 67° 49′, was so similar to that of the locality just described that it will require only a brief description.

Most of the Radiata of special interest have been mentioned in connection with the last locality. Among these were *Cerianthus borealis* V.; *Pennatula aculeata* Danielssen*; +(s) *Ophioglypha affinis* Lym. (Lutk. sp.), common but new to American waters; *Ctenodiscus crispatus* D. and K.; (s) *Archaster arcticus* Sars (young); (t, g, p, s) *Schizaster fragilis* (D. and K.), several large ones; (g, s) *Pentacta assimilis* (D. and K.); (s) *Thyone scabra* V. (new species)*; *Lophothuria Fabricii* V.

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not so numerous, less acute and lighter colored; the anterior part of the body has smaller and less prominent suckers or verrucæ; the skin is lighter colored, thinner, and more translucent, and there is a zone bearing several rows of minute, slender, acute, chitinous spinules, a little below the tentacles.

* Since printing the first part of this paper, I have been able to satisfy myself that the species there mentioned (p. 5) is the *P. aculeata*, of northern Europe. Dr. Kölliker regards this form, however, as a variety of *P. phosphorea*, but it seems to me sufficiently distinct. Mr. Whiteaves, in an article just received (Ann. and Mag. Nat. Hist., Nov., 1872, p. 346), has proposed for his specimens the name of *P. Canadensis*, influenced to some extent, undoubtedly, by my former opinion, based on his specimens only, that it was a distinct species. His specimens differ considerably from mine, or the typical *P. aculeata*, both in form and size, being much larger and more elongated, though having the same structural characters. The name *Canadensis* may be appropriately retained to designate this variety. The *Virgularia*, referred to in the same place, proves to be the same as the much larger and better specimens dredged by Mr. Whiteaves in 200 fathoms, and kindly loaned to me for examination. It appears to agree in all essential characters with *V. Lyngmanni* Köll., known before only from the Azores in 30 to 80 fathoms. It is quite distinct from *V. mirabilis*, being shorter and stouter, with a much shorter basal portion, and the polyps are much larger and fewer (only 5 or 6 in each cluster) while the lobes or wings are more elevated, deeply incised between the polyps, and they are not attached to the stalk so obliquely.

† *Thyone scabra*, sp. nov., (=*T. fusus?*, this Journal, p. 14; not of Koren).

Body fusiform, gradually tapered behind, with a long, slender, posterior portion, covered throughout with very numerous, rather rigid, slender, scabrous papillæ; skin rather rigid, scabrous with small rough points, which project from the plates. Tentacles ten; 8 large ones much elongated and arborescently divided from near the base; the two small ones are very short, nearly sessile, subdivided from the base. The calcareous plates of the skin are flat, somewhat imbricated, irregularly oval, triangular, or subpolygonal, with an undulated or crenulated margin, pierced by about 20 to 24, unequal round openings, two or three central ones larger than the rest, the interspaces mostly as wide as the pores; from the center of the upper side arises an open, slender, flat, acute spinous process, composed of two anastomosing pieces. The plates of the papillæ or suckers are narrow, elongated, bent into a bow-shape, the middle

Of Mollusca 33 species occurred, of which 20 were also und in 150 fathoms, and have been in part already indicated. mong the Bryozoa were (s) *Bugula avicularis, var. fastigiata* mitt; *Discofascigera lucernaria* (Sars, sp.)*; *Cellularia arctica* usk, sp.); (t, s) *Carberea Ellisii* Smitt. *Terebratulina septen-ionalis* and *Ascidiopsis complanata* V. also occurred. Among e Lamellibranchs were *Pecten Islandicus; P. tenuicostatus; nomia aculeata; Crenella glandula;* (s, t) *Astarte lens*, dwarf ar.; *Thracia myopsis; Cryptodon Gouldii;* +*Neæra obesa* Sars.; *ardium pinnulatum*. The most interesting of the Gasteropods ere +*Pleurotomella Packardii* V., sp. nov., described by error nder the previous locality; and (s) *Ringicula nitidia* V. new sp.; ut *Philine quadrata*, *Lepeta cæca*, *Velutina haliotoidea*, and *hiton mendicarius* also occurred, in addition to many of the ecies enumerated from 150 fathoms (s).

Of the Crustacea the most important were (u, x) *Caridion ordoni* Goes and +*Stegocephalus ampulla* Bell, both decidedly rctic species, the latter not before known from America, but redged also in the Gulf of St. Lawrence this summer, by Mr. hiteaves; *Harpina fusiformis* Smith (Stimp. sp.), *Unciola rorata* Say, and *Anthura branchiata* Stimp. also occurred.

The Annelids were even more numerous than at (s), and ere represented by 44 species, of which 23 were also found the former locality.

Among the additional species were +*Antinoë Sarsii* Malm.; *ereis pelagica* Linn.; *Leodice vivida* (Stimp. sp.); *Ammotrypane ulogaster* Rathke; *Sternaspis fossor* Stimp.; +(s) *Nicomache lum-icalis* Malm.; (s, p) *Praxilla gracilis* Malm.; (p) *P. prætermissa* alm.; *P. torquata* (Leidy sp.); + (t) *Notomastus latericius* ars, abundant; +(x) *Eumenia crassa* (?); (s, x, w) *Amphicteis unneri* Sars, common; (s, x, w) *Ampharete Finmarchica* Sars; *Samytha sexcirrata* Malm.; *Amphitrite cirrata* Müll; and ecies of *Phyllodoce*, *Eteone*, *Aphlebina*, *Nephthys*, *Rhynchobolus*, *mmochares*, and *Sabella*.

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panded and usually pierced by about four pores, two of which are larger; the ds are also usually dilated and pierced with small pores; from the middle arises flat spinous process, similar to that of the skin-plates, but smaller. Length, in cohol, about 2 inches; greatest diameter ·25 to ·35; length of longest tentacles 0. Color of preserved specimens yellowish brown.

Near St. George's Banks, in 110 and 150 fathoms.

This species resembles *T. raphanus* Dub. and Koren (=*T. cigaro* Trosch. sp.) in rm, but the latter has long-stalked tentacles, branching only near the ends, and e plates of the skin are different in form, and in the perforations, and lack the inous processes which give this species its rough scabrous surface.

* This is the *Defrancia lucernaria* of Sars. The name *Defrancia* (Bronn, 1835) as preoccupied for a genus of shells (Millet, 1826). I have therefore adopted *iscofasigera* D'Orb., the name which appears to be next in priority, for this genus. is species has not been recorded from our coast, but I have seen another speci-n from Nova Scotia (Willis), and Mr. Whiteaves has also dredged it in the Gulf St. Lawrence.

Four Sipunculoids occurred, viz.: *Phascolosoma cæmentarium* (Qf.); (s. p. x) *P. tubicola* V., abundant; +*P. borealis* Kef. (?)*; and *Chætoderma nitidulum* Lov. (?); the last named species was also dredged by me in Passamaquoddy Bay, 30 fathoms, but has not been known before from the American coast; it is a very remarkable worm, thickly covered over the whole surface with slender, shining spines, directed backward.

One of the commonest species, both here and in 150 fathoms (s), was a new and beautiful species, which I have called *Nothria opalina*† in allusion to its brilliant opal-like iridescence. It is nearly allied to *N. conchylega*, which also occurs with it, but is a more slender and depressed species, and in addition to many other characters it differs remarkably in color, the latter being conspicuously banded tranversely with dark red and bluish white. It also constructs a different kind of tube, for while the *conchylega* forms a parchment-like inner tube to which it firmly attaches coarse gravel, together with fragments of shells, flat pebbles, etc., in such a way as to form a broad, flat, heavy outer protection to the tube, the *opalina* cements to its inner parchment-like tube only a thin covering of mud or fine sand, thus forming a long, slender, round tube, resembling that of a *Sabella*. In respect to its tube it is, therefore, intermediate between *N. conchylega* and *Hyalinœcia tubicola* Malmg., which forms a thin transparent tube, without any external protecting layer of foreign materials. *Samythella elongata* V., was also common here, as well as *Goniada maculata* Œrsted,* *Maldane Sarsii* Malm., and *Melinna cristata* Malmgren.†

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* This species is rather short and thick, obtuse posteriorly, nearly smooth to the naked eye, and destitute of both hooks and distinct suckers, but the skin is minutely wrinkled transversely, and covered with almost microscopic slender papillæ, and is minutely specked with dirty yellowish brown; the retractile portion is more distinctly graunlated anteriorly. The tentacles are rather numerous, small and simple. Mr. Whiteaves has also dredged it in the Gulf of St. Lawrence this year.

† *Nothria opalina*, sp. nov.

Body long and slender, narrowed anteriorly, much depressed and of nearly uniform width throughout most of its length; the five anterior segments much longer than the others. Palpi inferior, rather large, hemispherical; antennæ small, ovate, close together, on the front of head. Three central tentacles very long and slender, tapering, acute, the basal portion regularly annulated and thickened for a considerable distance, beyond which the surface is smooth, with an occasional distant annulation; the central odd one is somewhat shorter and more slender than the two adjacent ones, which reach to or beyond the 10th segment; outer pair much shorter, being less than half the length of the central ones. Tentacular cirri small and very slender. Lateral appendages or "feet" of the first six setigerous segments similar in structure but more prominent than the following ones, from which they also differ in having the ventral cirrus well developed, long and tapering, but shorter and thicker on the first segment than on the five following. Those of the first pair have a stout stalk, which terminates in a small bluntly rounded setigerous lobe, with a long, slender, subterminal cirrus-like lobe above, longer than the stalk; dorsal cirrus arising from near the base, longer and more slender than the terminal cirrus; branchial filiament simple, long and very slender, about equalling the dorsal cirrus and united to it above its base; ventral cirrus ovate, tapering, blunt, arising from near the base. The second pair of feet are similar to those of the first, except that in the largest specimens there are two branchial filiaments, and the ventral cirrus is longer and more slender. The 3d, 4th, 5th and 6th pairs have essentially the same structure, but the ventral cirrus becomes gradually longer to the 6th, where it is longer than the stalk and nearly equal to the terminal cirrus. The succeeding feet are much shorter; the ventral cirrus is a mere conical papilla, which soon disappears; the terminal cirriform lobe becomes smaller and disappears after the 10th pair; the branchial filiament becomes larger and longer to the middle region, where it exceeds in length half the diameter of the body, while the dorsal cirrus at the same time becomes smaller and shorter, until it is less than one-fourth the length of the branchia.

The setæ of the anterior feet consist of slender, acutely pointed, curved ones, mixed with much stouter, blunt pointed compound ones; farther back there are two fascicles of more slender acute setæ, and in the lower bundles a few long, stout, bidentate hooks, with a thin, rounded, terminal expansion.

Color in alcohol, pale yellowish white, but everwhere very brilliantly iridescent with opaline luster and colors.

Length, 2·5 inches; diameter, ·10.

Near St. George's Banks in 110 and 150 fathoms, common.

From the 85-fathom locality (p) N. lat. 42° 3′, W. long. 67° 45′, very few species were obtained in addition to those from the two localities described above, but many of the deep-water species already mentioned occurred, which we should hardly have expected in waters so much shallower. *Cerianthus borealis* V., a species of *Edwardsia*, *Pennatula aculeata* Dan., and *Schizaster fragilis* were among the Radiata. Among the Mollusca were *Yoldia obesa*, *Y. thraciformis*, *Scalaria Grœnlandica*. There were also several Crustacea and numerous Annelids of species already indicated. Among the latter were a few additional ones, viz.: *Nephthys discors* Ehl.; *N. ingens* Stimp.; (o) *Praxilla prœtermissa* Mal.; a very slender undetermined *Clymene* or *Praxilla*, inhabiting almost capillary tough tubes, covered with fine sand, which occurred in great numbers; + *Chœtozone setosa* Malmgren (?). Our specimens of the last named species are more slender than represented by Malmgren, with longer setæ, and the posterior segments are more deeply incised.

The fauna of the two remaining localities in 45 fathoms (q), N. lat. 42°, W. long. 67° 42′; and in 40 fathoms (r), N. lat. 42° 3′, W. long. 67° 31′, was nearly identical with that obtained on the same kind of sandy and shelly bottoms at similar depths on the banks farther to the southwest, by Messrs. Smith and

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* Our specimens agree with those figured and described by Ehlers, (Borstenwürmer, p. 704, Tab. xxiv, figs. 36 to 48), having a circle of 18, short, fleshy papillæ at the end of the proboscis, and a prehensile apparatus within, consisting of nine pieces, viz.: a pair of large black jaws with eight fangs; a pair of smaller ones with three fangs; an odd median smaller one with three fangs; and two pair of distant, small, slender ones. There are 7 to 9, V-shaped pieces in each of the rows toward the base of the proboscis. Œrsted, Johnston, Quatrefages, and others have described this species as destitute of the terminal armature of the proboscis.

† The specimens dredged by Dr. Packard differ somewhat from Malmgren's figures and description. The body is longer and more slender; the unguiform hooks, back of the branchiæ, are more strongly curved, with a more slender and acute point; the transverse serrated border, behind the branchiæ, has 13 teeth. In most other respects they agree well. The tube is covered with gravel, each grain being loosely attached by one end.

Harger, and already partially described (see pages 9 and 10, localities e, d, c). The most characteristic feature of this sandy-bottom fauna of the higher parts of the Bank is the great abundance of Hydroids, Bryozoa, and Sponges. But numerous shells and Crustacea, mostly of common New England species, also occurred. Of Crustacea there were 22 species, from localities f and r, among which were the spider-crab, *Hyas coarctatus;* common sand-crab, *Cancer irroratus;* the hermit-crabs, *Eupagurus Bernhardus*, *E. pubescens*, and *E. Kroyeri;* the common shrimps, *Crangon vulgaris*, *Pandalus annulicornis*, and *Hippolyte pusiola;* the Amphipods, *Phoxus Kroyeri* Stimp., *Paramphithoë cataphractus* Smith (St. sp.), *Unciola irrorata* Say., *Cerapus rubricornis* Stimp.; with *Balanus porcatus*, etc. The Annelids were not numerous and mostly of common species; among them were *Thelepus cincinnatus* Malm. (=*Lumara flava* Stimp.), several species of *Sabella*, *Nereis pelagica*, *Leodice vivida* V. (Stimp. sp.), *Nothria conchylega* Mal., and a brilliantly phosphorescent species, belonging to the *Syllidæ*, which inhabits tubes attached to Hydroids. Most of the Hydroids and Bryozoa were the same as those mentioned as from locality (d) on page 10. The only species of special interest were +*Halecium labrosum* Alder, new to American waters; (e) *Campanularia* (*Orthopyxis*) *Hincksii* (Alder); (e) *Coppinia arcta* Hincks; and *Grammaria abietina* Sars (= *G. robusta* Stimpson).*

Of Echinoderms only the more common northern species occurred; such as *Asterias vulgaris*, *Solaster endeca*, *Crossaster papposus*, *Euryechinus Dröbachiensis*, *Echinarachnius parma*, and several species of the common Ophiurans.

General Results from the Explorations by the Bache.

Before discussing the work done in the Bay of Fundy, it will perhaps be best to consider the principal conclusions that may be drawn from the facts already presented in regard to the faunæ and physical conditions of St. George's and Le Have Banks and the regions adjacent.

It is evident that the collections represent several distinct faunæ and sub-faunæ. For our present purposes they may be grouped under six heads. The following are then some of our conclusions:

1st. The surface-fauna outside of the banks, and at certain times even over their outer slopes, belongs to the peculiar fauna prevailing over the entire surface of the central parts of the Atlantic Ocean, and shows very clearly the direct effects of the Gulf Stream.

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* Mr. Hincks considers this genus identical with *Salacia* of Lamouroux. To me, however, they seem to be widely different. Be this as it may, it seems hardly worth while to unite them until the Australian species, for which the genus *Salacia* was established, is better known.

2d. The surface-fauna inside of the banks is decidedly northern in character and very similar to that of the Bay of Fundy. Contrasted with the preceding, it shows that the Gulf Stream is almost entirely turned aside by the banks, and has comparatively little effect upon the fauna between them and the coast.

3d. The fauna of St. George's Bank itself is decidedly boreal in character, and aside from the absence of all littoral species, is essentially identical with that found in the Bay of Fundy at corresponding depths, on similar bottoms, in regions swept by strong currents. The fauna of the southwestern parts is, however, somewhat less boreal in character than that of the northeastern. The bottom is generally composed of sand or gravel, with broken shells, etc.

4th. The fauna on Le Have Bank, and off Halifax, even at the moderate depth of 20 fathoms, is decidedly more arctic in character than that of St. George's Bank or the Bay of Fundy at similar or even greater depths.

5th. Between St. George's and Le Have Banks and the coast there is a great region of cold and comparatively deep water, in places more than 100 fathoms deep, having a bottom of mud and fine sand over large areas, and communicating with the great ocean-basin by a channel between St. George's and Le Have Banks, which is comparatively narrow and deep, in places at least 150 fathoms.

This partially enclosed region has, physically and zoölogically, the essential features of a gulf, and might be appropriately called St George's Gulf. The deep waters of the Bay of Fundy are directly continuous with those of this area.

The fauna, found upon the muddy bottom of this gulf and its outlet, is peculiarly rich in species new to our coast, and is nearly identical with that of the deeper parts of the Gulf of St. Lawrence, and also agrees very closely with that found on muddy bottoms and at similar depths on the coasts of Greenland, Finmark, and Norway.

6th. The deepest dredging (g), in 430 fathoms, was outside of St. George's Bank, on the slope of the actual continental border, and within the limits of the true Atlantic "basin." The fauna there is evidently rich and varied, decidedly northern in character, and agrees closely with that of similar localities and depths on the European side. The bottom was chiefly sand and gravel of varied materials, with many small rounded boulders, and probably patches of mud, indicating, apparently, the existence of currents sufficiently powerful to prevent the accumulation of fine sediment. The existence of extensive "rips" even farther off and in deeper water (see page 13), seems to confirm this. The animals were mostly such as inhabit bottoms swept by strong currents in the Bay of Fundy.

7th. Everywhere over the banks, and especially over their southern slopes, there is a great difference between the temperature of the bottom and surface, generally amounting to 15°-20° F., or even more. The temperature of the surface was generally from 60° to 72°. The temperature of the air was always very near that of the water; but generally one or two degrees higher. Owing to the great difference between the temperature of the bottom and surface waters, it was found impossible in most cases to keep the animals brought up in the dredge alive, the warm surface waters proving fatal in a very short time.

8th. Inside of the banks, both in the Bay of Fundy and in St. George's Gulf, near the banks, no such great contrast in the temperature of the bottom and surface was found, the difference being seldom more than 10°, and often, especially in the Bay of Fundy, less than 5°. The surface temperatures, at corresponding dates, in the Bay of Fundy were 48° to 53°, showing an avarage difference of about 20° for the surface temperatures in the two regions, while the average bottom temperatures do not appear to differ materially.

9th. The high surface temperature of the Banks is evidently due chiefly to the direct influence of the Gulf Stream. Its heated water impinges against their outer slopes, but cannot pass in any considerable volume beyond them.

10th. The very low surface temperature of the Bay of Fundy is due largely to its *geographical position* and the *absence* of any appreciable influence from the Gulf Stream; but is no doubt intensified by the powerful tides which are constantly mixing up the cold bottom water with that of the surface. The prevalence of foggy and cloudy weather over this region of cold water during summer, by obstructing the heat of the sun, may also add to the effect, but this must be partially neutralized by the action of the fog and clouds in preventing the radiation of heat.

The facts hitherto observed do not seem to warrant the assumption that an "arctic current," properly so-called, as distinguished from the tidal currents, enters St. George's Gulf or the Bay of Fundy. The action of the tidal currents, in bringing up the cold bottom waters of the ocean, is perhaps a cause sufficient to produce most of the coldness of the water in this region. We may suppose, however, that these waters constantly receive, in the tidal currents, accessions of cold water, which has primarily come from the north in the arctic current. It should be added that we do not yet know the temperature which would be produced in a similar body of water in this region by the climatic conditions alone, independently of tides and currents.

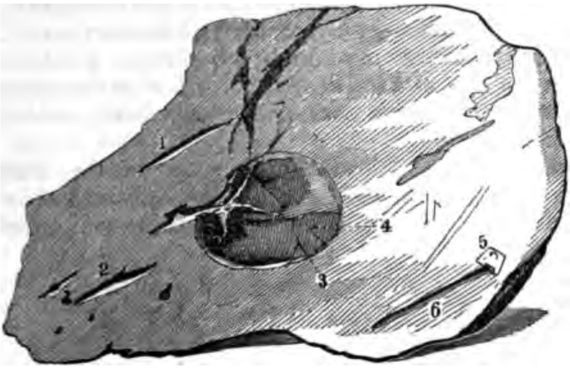
Errata.—In the first part of this article, pp. 5 and 14, for *Astropecten*, read *Archaster*. Page 9, line 30, for 65° 50.3′, read 65° 58.3′.

(To be continued.)

Art. XIII.—*A description of the Victoria Meteoric Iron, seen to fall in South Africa in* 1862, *with some notes on Chladnite or Enstatite;* by J. Lawrence Smith, Louisville, Ky.

The Victoria Meteoric Iron, although found about ten years since, has never been described; and yet it is one of the most interesting of this class of metorites. I have succeeded in collecting the following facts in connection with it. It was seen to fall in the year 1862 by a Dutch farmer in Victoria West, Cape Colony, South Africa, and was given by him to Mr. Auret the Civil Commissioner of that district, who presented it to the South African Museum at Capetown.

Although it is an iron which has a tendency to decompose rapidly, it has not undergone the decomposition that would have inevitably taken place had it long remained exposed to atmospheric influences. This fact, coupled with another that the farmer could have no object to deceive any one with reference to a body which certainly bears evidence that it fell at some time from the heavens, and as those who know the farmer have every confidence in his statement, we are led to conclude that it is to be placed along side of the Agram, Brannau and Dickson county irons. The mass was pear-shaped, and weighed about 6 lbs. 8 oz. One end was smooth and rounded: the smaller end was jagged as if torn or parted from a larger meteorite.



A small fragment of it was first brought to Europe in 1868, by which its true meteoric character was established. In 1870 the trustees of the South African Museum had it cut, retaining one half of it, and sending specimens to the British, Calcutta, Vienna and Berlin Museums, also to Mr. Nevill of Godalming, and a mass weighing about twelve ounces to myself.

From the specimen in my possession I here describe its characteristics. The iron is compact, with a tendency to fissure near some portions of its surface. The amount of oxide on the surface is small, the cut surfaces showing bright metal quite up to the exterior surface. The Widmannstättian figures developed are of that class where the lines are delicate and straight, inclined at a considerable angle to each other, a form I have seen common to irons rich in Schreibersite. This last mineral is diffused through the iron in masses with absolutely straight boundaries, some of them $\frac{5}{8}$ to $\frac{3}{4}$ of an inch long by $\frac{1}{8}$ of an inch broad (1 and 2 on figure), and others much longer (6) and narrower, others again triangular (5) and arrow-shaped; and on my specimen a layer of it (3) coating an oval cavity that must have been an inch and a half in its longest, and one inch in its shortest diameter, the Schreibersite having a thickness of about $\frac{1}{20}$ of an inch; the rest of this cavity being filled by pyrite (this is distinctly seen in the photograph). My specimen shows one fourth of this cavity and there are doubtless others in the original mass. The specific gravity is 7·692. On analysis it was found to contain:

Iron,	88·83
Nickel,	10·14
Cobalt,	·53
Copper,	minute quantity.
Phosphorus,	·28
	99·78

Enstatite or Chladnite.—This mineral now occupies so important a relation to the mineral constitution of meteoric stones that it is well to give an account of its discovery, and the subsequent investigations of different observers. Its discovery is beyond doubt due to Prof. C. U. Shepard, who first described it in the American Journal of Science, Sept., 1846, p. 381, calling it chladnite.

It constituted nearly the entire mass of the Bishopville meteorite that fell in 1843. Prof. Shepard did not make out its composition correctly, his analysis being imperfect. The composition given by him was

Silica,	70·41	3 of oxygen.
Magnesia,	28·25	1 " "
Soda,	1·39	

making it out to be a tersilicate of magnesia. Although the constitution was incorrectly determined, Prof. Shepard clearly showed that it differed in character from any then known mineral.

Eight years after the mineral was first made known, a small fragment of the meteorite coming into my possession, a reëxamination was made of its chemical constitution, and the errors ıe first analysis discovered. But not having enough of the ›orite for analysis, the simple statement was presented to American Association for the Advancement of Science in il, 1854, "that from some investigations just made, chlad- is likely to prove to be a pyroxene." This was noticed in Proceedings of the Association for that year, and referred n the American Journal of Science, March, 1855, p. 162. years later a specimen of the Bishopville meteorite of good being placed at my disposal, the mineral was separated in ry pure state and found to be composed as follows:

Silica,	59·97
Magnesia,	39·33
Peroxide of iron,	·40
Soda with feeble potash and H,	·74
	100·44

he minute quantity of peroxide of iron came from a little ıllic iron that was present. The analysis afforded the gen ratio 2 : 1, corresponding to the formula $\dot{M}g^3$, $\dddot{S}i^2$ (or $\dddot{S}i$,) corresponding to the general formula of pyroxene. details of the examination then made, are to be found in American Journal of Science, Sept., 1864,. where it is her stated that chladnite approaches those forms of pyroxene wn as white augite, diopside, white coccolite, &c., these named minerals having part of the magnesia replaced by ı. It is identical with the enstatite of Kenngott, a pyrox- mineral form Aloysthal in Moravia.

rom these observations it will be seen that the Bishopville ›oric stone, however different in external characteristics ı other similar bodies, is, after all, identical with the great ly of pyroxenic meteoric stones.

nstatite.—This form of pyroxene was first noticed by ngott as a new species, in a communication made by him to Vienna Academy in 1855, (see Vien. Acad. Ber., xvi, p. 162, ·esbericht for 1855, p. 928.) Its composition there given is—

Silica,	57·09
Alumina and oxide iron,	5·13
Magnesia,	35·85
Water,	1·92
	99·99

he crystallographic character of this mineral entitles it to ration from pyroxene, or in other words, as it is entitled to ınked as a new species, the prior right of discovery belongs rof. Shepard, and the name first given by him, chladnite, :he priority; but as it has for so long time borne the name nstatite among minerologists, any attempt to change it would only bring confusion. This is the more to be regretted, since the name of Chladni would be a most appropriate affix to a mineral, the true and pure type of which is so preëminently that in meteorites.

In this connection I would refer to the simple chemical relation of three of the most characteristic minerals of meteoric stones; these minerals forming at least 90 per cent of the earthy minerals in the aggregate mass of all meteoric stones. The three minerals are:

Enstatite,	$\dot{R}\,\dddot{S}i$	$\dot{M}g\ \dddot{S}i.$
Bronzite,	$\dot{R}\,\dddot{S}i$	$(\dot{M}g\ \dot{F}e)\ \dddot{S}i.$
Chrysolite,	$\dot{R}_2\,\dddot{S}i$	$(\dot{M}g\ \dot{F}e)\ \dddot{S}i.$

In these minerals, the protoxide of iron replaces but a small portion of the magnesia in the last two; so they are virtually silicates of magnesia containing one or two atoms of silica with one atom of magnesia.

ART. XIV.—*Analytical Notices;* by WOLCOTT GIBBS, M.D.

1. *On the quantitative estimation of chromium and the separation of chromium from uranium.*

THE quantitative separation of chromium from uranium appears not to have specially attracted the attention of chemists. No method is given either by Rose or by Fresenius. The two metals rarely, if ever, occur associated in the mineral kingdom, and the only definite artificially prepared compound which I have been able to find noticed is the uranic chromate described by Jahn, who does not appear to have analyzed the salt, though Berzelius—judging probably from the mode of formation—attributes to it a formula which we should now write $U_2\Theta_2 \,.\, Cr\Theta_4$. Berzelius also states that neutral potassic chromate gives with uranous chloride a yellowish-brown precipitate, which contains both oxides of uranium as well as chromic oxide and acid. This compound also appears not to have been analyzed.

As the method of separating the two metals to which I finally arrived involves the presence of the chromium as chromic acid, I began my investigation by examining the commonly received methods of estimating this substance.

Rose strongly recommends the method of Berzelius, which consists in precipitating the chromic acid by mercurous nitrate, and washing with a dilute solution of the same salt. The precipitated chromate is voluminous, and has a brown-red color when the precipitation takes place in the cold. I find that a better result is obtained by precipitating at a boiling heat, when the mercurous chromate almost immediately becomes highly crystalline, its color changing to a bright scarlet. It may then be washed with the greatest ease, and ignited in the usual manner. It is absolutely necessary in applying this method that the mercurous nitrate used should be perfectly free from nitrous acid. Want of attention to this point led me formerly into an error, which I desire to correct in this place. I have stated in a former paper* that hot solutions must not be employed on account of the reduction of chromic acid by mercurous nitrate. This reduction is not due to the temperature, but to the presence of a small quantity of nitrous acid in the mercurous nitrate employed. It is easy to avoid this source of error by dissolving the mercury in nitric acid, in an open vessel, and crystallizing the nitrate two or three times, using for solution dilute nitric acid which has been perfectly freed from nitrous acid by a current of air or carbonic dioxide.

To test the method thoroughly, the following analyses were made with pure potassic dichromate:

I. Salt precipitated at a boiling heat by mercurous nitrate and washed with hot water alone.

1. 0·6003 gr. gave 0·3030 gr. $\text{Cr}_2\Theta_3 = 50{\cdot}47\%\ \text{Cr}_2\Theta_7$.
2. 0·4741 gr. " 0·2407 gr. " $= 50{\cdot}77\%$ "

The formula $\text{K}_2\text{Cr}_2\Theta_7$ requires $51{\cdot}73\%$ if we take $\text{Cr} = 52{\cdot}2$.

II. Salt precipitated cold by mercurous nitrate and washed with cold water only.

3. 0·2641 gr. gave 0·1344 gr. $\text{Cr}_2\Theta_3 = 50{\cdot}89\%\ \text{Cr}_2\Theta_7$.
4. 0·5098 gr. " 0·2607 gr. " $= 51{\cdot}13\%$ "

III. Salt precipitated cold, then boiled and washed with boiling water only.

5. 0·4957 gr. gave 0·2503 gr. $\text{Cr}_2\Theta_3 = 50{\cdot}49\%\ \text{Cr}_2\Theta_7$.
6. 0·6393 gr. " 0·3288 gr. " $= 51{\cdot}43\%$ "

IV. Salt precipitated cold, then boiled and washed with hot water containing mercurous nitrate.

7. 0·4951 gr. gave 0·2558 gr. $\text{Cr}_2\Theta_3 = 51{\cdot}67$.
8. 0·3639 gr. " 0·1881 gr. " $= 51{\cdot}69$.

In these last analyses, the error of the mean is only 0·04%. We arrive, however, more quickly at our object when we precipitate at once at the boiling point, and then wash with a hot dilute solution of the nitrate.

In several works on Analytical Chemistry it is recommended to precipitate chromic acid from its solutions by plumbic acetate, and to weigh the resulting chromate of lead. In repeated trials, I have never been able by any artifice whatever to prevent the precipitated plumbic chromate from passing more or less through the filter so as to render the filtrate turbid.

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* This Journal [II], vol. xxxix, p. 59.

Precipitation of chromic acid by a baric salt was next examined. Potassic dichromate was precipitated by baric acetate, with the following variations:

I. Salt precipitated by baric acetate at a boiling heat, and washed with water only; chromate weighed upon a porous earthenware filter.

1. 0·4617 gr. gave 0·7894 gr. $\text{BaCr}\Theta_4$ = 51·41%.
2. 0·4685 gr. " 0·8022 gr. " = 51·52%.

II. Salt precipitated by baric acetate at a boiling heat, alcohol added, and the precipitate washed with a hot mixture of 3 parts water and 1 part alcohol of 90% and ignited.

3. 0·3802 gr. gave 0·6546 gr. $\text{BaCr}\Theta_4$ = 51·78%.
4. 0·5282 gr. " 0·9069 gr. " = 51.66%.

III. Salt precipitated by baric acetate without alcohol. Solution after precipitation evaporated to dryness upon a water bath, then washed with hot water and ignited.

5. 0·5366 gr. gave 0·9229 gr. $\text{BaCr}\Theta_4$ = 51·75%.
6. 0·5355 gr. " 0·9204 gr. " = 51·71%.

In the last analysis alcohol was added to the wash-water. From this it appears that very accurate results may be obtained by precipitation with baric acetate at a boiling heat, adding a small quantity of strong alcohol to the liquid, washing with water containing alcohol, and igniting. The wash-water need not contain more than $\frac{1}{12}$ of its volume of alcohol. The precipitated chromate must, before filtering, be allowed to settle completely, leaving the supernatant liquid perfectly clear. The filtrate never becomes turbid even after all the soluble salts are washed out. Finally, it is not necessary to weigh the baric chromate upon a weighed filter. A very small quantity of the chromic acid is always reduced by the carbon of the filter in igniting, but the loss of weight is inappreciable. This method is much shorter than that which is usually employed, as the filtration and washing may be executed almost immedidiately after precipitation.

The conditions necessary for the complete precipitation of chromic acid, either as mercurous or baric chromate, having been thus carefully reviewed, I next proceeded to attempt the quantitative separation of uranium and chromium. In a first series of experiments weighed quantities of potassic dichromate were mixed with much larger but undetermined quantities of uranic nitrate. The chromic acid was then precipitated by mercurous nitrate from the boiling solutions. In this manner the following results were obtained:

1. 0·4120 gr. $\text{K}_2\text{Cr}_2\Theta_7$ gave 0·2130 gr. $\text{Ꞓr}_2\Theta_3$ = 51·74% $\text{Cr}_2\Theta_7$.
2. 0·3292 gr. " " 0·1702 gr. " = 51·70% "
3. 0·4543 gr. " " 0·2353 gr. " = 51·77% "

The mean of these analyses is 51·73%, which is precisely the percentage required by the formula $K_2Cr_2\Theta_7$. ($Cr = 52·2$).

These analyses show that mercurous nitrate gives very accurate results. The employment of this salt in separating chromium from uranium is indicated only in those cases in which the chromium exists as chromic acid, in which relatively small quantities of chlorine or sulphuric acid are present, and in which no other acid is present which, like phosphoric acid, gives an insoluble mercurous salt not completely volatilized by ignition. In the presence of chlorine, sulphuric acid, &c., the following process may be very advantageously employed. The solution is to be boiled for a few minutes with a small excess of sodic hydrate, the precipitate of sodic uranate filtered off and washed with hot water containing a little sodic hydrate until the washings no longer give any turbidity, with a solution of mercurous nitrate. The sodic uranate in the filter is then to be dissolved in chlorhydric acid, and the uranium determined in the usual manner. The filtrate contains all the chromium as $\mathrm{Є}r\Theta_4Na_2$. After adding chlorhydric acid in excess, the chromic acid may be most conveniently reduced to chromic oxide by adding a solution of potassic or sodic nitrite and boiling for a few minutes, after which the oxide may be precipitated by ammonia in the usual manner. An alkaline nitrite is a better reducing agent than alcohol, as the chromic oxide may be precipitated immediately after the reduction.

It remains to consider the case in which chromic and uranic oxides occur together in solution. A solution of sodic hydrate in small excess is to be added, and the whole heated to boiling. To the hot liquid bromine water is to be added. Chromic oxide is almost instantly oxidized to chromic acid, which remains in solution as $\mathrm{Є}r\Theta_4Na_2$, while uranate of soda with a small percentage of uranic chromate remains undissolved. After washing with hot water containing a little sodic hydrate, the precipitate, which has a deep orange color, is to be dissolved in hot nitric acid, the solution boiled for a few minutes to expel any traces of nitrous acid, mercurous nitrate added, and the whole allowed to stand until the small quantity of mercurous chromate has settled. This, after washing, may be ignited in the same crucible with the chromic oxide obtained as above from the sodic chromate in the filtrate. The filtrate is free from uranium. Repeated attempts to determine uranium by precipitation with sodic phosphate and final weighing as uranic pyro-phosphate, have led as yet to no satisfactory results. It is, however, worth noting that the gelatinous phosphate becomes pulverulent, and easily washed by simple evaporation to dryness.

2. *On the estimation of magnesium as pyro-phosphate.*

All works on quantitative analysis recommend the precipitation of magnesium in the form of ammonia-magnesic phosphate, from cold solutions, by disodic phosphate. I find it more convenient, if not more accurate, to employ microcosmic salt as a precipitant, and to precipitate from concentrated and boiling solutions. After cooling ammonia is to be added, and the process then continued in the usual manner. The following analyses were made under my direction by Mr. C. E. Munroe to test the method. In the first series pure magnesic sulphate was precipitated at a boiling heat and in concentrated solutions by microcosmic salt, no ammonic chloride being present.

1. 0·6430 gr. gave 0·2914 gr. $Mg_2P_2\Theta_7 = 9{\cdot}85$.
2. 1·1523 gr. " 0·5210 gr. " $= 9{\cdot}79$.
3. 0·7064 gr. " 0·3181 gr. " $= 9{\cdot}78$.
4. 0·8081 gr. " 0·3666 gr. " $= 9{\cdot}80$.

The formula $S\Theta_4Mg+7\Theta H_2$ requires 9·76%. The mean of the four analyses is 0·04% too high. In a second series the same process was employed, but ammonic chloride was added to the magnesic solution before precipitation. In this manner:

5. 0·5448 gr. gave 0·2461 gr. $Mg_2P_2\Theta_7 = 9{\cdot}76$.
6. 0·6684 gr. " 0·3026 gr. " $= 9{\cdot}78$.
7. 0·7610 gr. " 0·3442 gr. " $= 9{\cdot}78$.
8. 0·6408 gr. " 0·2906 gr. " $= 9{\cdot}79$.

The mean of the four analyses gives 9·78%, or 0·02% too high. Two analyses were then made by precipitating the boiling solution of disodic phosphate after adding ammonic chloride. In this manner:

9. 0·5407 gr. gave 0·2536 gr. $Mg_2P_2\Theta_7 = 10{\cdot}13\%$.
10. 0·8305 gr. " 0·3881 gr. " $= 10{\cdot}10\%$.

This method must therefore be wholly rejected, the mean error being +0·35%.

The same process was then repeated, only the precipitated ammonia-magnesic phosphate at first obtained, after addition of ammonia water and perfect subsidence, was redissolved in dilute chlorhydric acid, and again precipitated by ammonia. In this manner:

11. 0·5916 gr. gave 0·2686 gr. $Mg_2P_2\Theta_7 = 9{\cdot}79\%$.
12. 0·7371 gr. " 0·3340 gr. " $= 9{\cdot}79\%$.

The error is here only +0·03%, but the method is longer in its application and less convenient than that given above with microcosmic salt. This last may, I find, be used with equal advantage in precipitating manganese from hot solutions. The precipitate is crystalline, and the process is more convenient than that which I formerly gave. A little ammonia should be added to the solution before filtering.

On the estimation of cobalt.—The extraordinary stability of the cobaltid-cyanide of potassium, $\text{Co}_2\text{Cy}_{12}\text{K}_6$, enables us to separate cobalt advantageously from many other metals by bringing it into this form. Wöhler first proposed to precipitate the double cyanide by mercurous nitrate, and to weigh the cobalt finally as metal. I find it particularly advantageous to precipitate at a boiling heat, and then boil for a few minutes with mercuric oxide so as to neutralize as completely as possible any traces of free nitric acid. By precipitating from hot solutions a granular, crystalline, mercurous salt is obtained, which is very readily washed. The following analyses were made to test the method. In (1) and (2) the precipitation was effected at a boiling heat, and the precipitate was simply washed with hot water containing a little mercurous nitrate. The cobalt was, after careful ignition with free access of air, finally reduced in hydrogen. The salt employed was pure crystallized $\text{Co}_2\text{Cy}_{12}\text{K}_6$.

(1.) 0·5063 gr. gave 0·0890 gr. cobalt = 17·57%.
(2.) 0·6785 gr. " 0·1197 gr. " = 17·64%.

The filtrate, after evaporation to dryness and ignition, gave with borax, before the blow-pipe, an extremely faint reaction for cobalt. In analyses (3) and (4) the solution was boiled with $\text{Hg}\Theta$ in small excess before filtration.

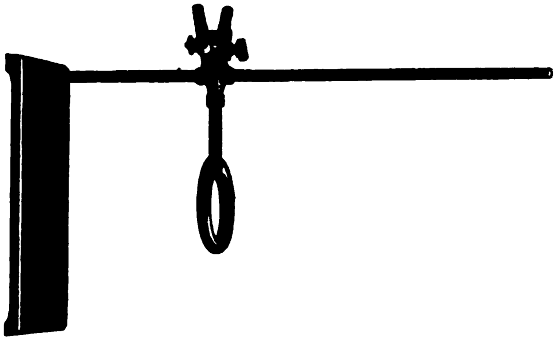
(3.) 0·5332 gr. gave 6·0947 gr. cobalt = 17·76%.
(4.) 0·6218 gr. " 0·1101 gr. " = 17·71%.

In (5) mercuric chloride was first added to the solution, and afterward sodic hydrate, until $\text{Hg}\Theta$ remained undissolved on boiling.

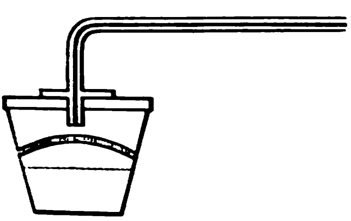
(5.) 0·5855 gr. gave 0·1035 gr. cobalt = 17·68%.

The formula requires 17·76%.

On some forms of laboratory apparatus.—Every chemist knows how difficult it is to conduct an evaporation quantitatively in a porcelain or platinum crucible heated from beneath. The following simple contrivance which I devised some years since, and which has long been in successful use in many laboratories in this country, deserves I think to be more widely known. It consists, as shown in the figure, of a hollow ring of metal, which can be moved up and down upon a vertical rod also of metal, and which is provided with two stop-cocks, by means of which air and gas may be admitted to the interior of the ring in proper proportions. The ring has a series of fine openings so placed that the little blue jets of burning gas point radially toward its center. The common water-blast may be employed with great advantage to give a continued supply of air, and when the proper proportions of air and gas are obtained—which requires but an instant—the little tongues of blue flame remain constant for hours. A foot bellows may also be employed when necessary. The crucible to be heated is supported upon the bottom of an inverted Beaufsay crucible. The ring-burner is then adjusted so that the points of the little jets of flame play upon the upper edge of the crucible to be heated. After a short time the ring-burner may be lowered so as to heat a lower zone of the crucible, and so on until the outer rim of the bottom is ignited. In evaporations the ring must be more slowly lowered. With a very little practice solutions even of sodic chloride may be evaporated to perfect dryness without loss by decrepitation. Loss by the creeping of solutions over the edges of the crucible is also prevented completely. In short, very numerous operations may be performed with the ring-burner more easily, quickly and safely than by any other form of apparatus with which I am acquainted.*



Another petty contrivance which I find of great service consists simply of a circular disc or meniscus of porous earthenware. In crucible ignitions, in which a current of gas is passed over the ignited substance—as for instance, in reducing metallic oxides in hydrogen—great care must be taken to prevent mechanical loss. In such cases I place a porous capsule in the crucible above the substance to be heated, as in the figure. The gas may then be introduced through the perforated cover by means of a porcelain pipe in the usual way, and passes through the porous capsule by diffu-



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* Ring-burners with stands, and two rings of different diameters, may be had of Messrs. Rohrbeck & Goebeler, 4 Murray street, New York.

sion. Mechanical loss is thus completely prevented, as the soft capsule may readily be filed so as to fit the crucible accurately.*

My acknowledgments are due to Mr. W. E. Cutter for his most efficient aid in the prosecution of my work.

December 16th, 1872.

ART. XV.—*On the gigantic fossil Mammals of the Order Dinocerata*; by O. C. MARSH. With plates I. and II.

AMONG the many extinct animals of interest hitherto discovered in the Tertiary of the Rocky Mountain region, none, perhaps, are more remarkable than the huge mammals which have recently been described from the Eocene beds of Wyoming. It is important, therefore, that accurate information in regard to them should be promptly made public, especially as serious errors on this subject have already appeared in various scientific publications, and are being widely disseminated.

These animals nearly equalled the elephant in size, and had limb bones resembling those of Proboscidians, as stated in the original description of the type species, *Tinoceras anceps* Marsh. The skull, however, presents a most remarkable combination of characters. It is long and narrow, and supported three separate pairs of horns. The top of the skull is deeply concave, and on its lateral and posterior margin there is an enormous crest. There were large decurved canine tusks resembling those of the walrus, but no upper incisors. The six premolar and molar teeth are quite small. Several species of these remarkable animals have already been named, but at present they cannot all be distinguished with certainty. The type species of the group (*Tinoceras anceps* Marsh) was based on the specimen first discovered; which was found by the Yale College party in September, 1870, and described by the writer in June, 1871, under the name *Titanotherium ? anceps.*† To this description, the authors who have since described the same, or allied, species have not referred. In the following year Prof. Cope gave the name, *Loxolophodon semicinctus*, to a single premolar tooth, which perhaps belongs to this group, and may prove to be identical with the above species.‡ In August last, in a paper issued in advance of the Proceedings of the Philadelphia Academy, Dr. Leidy described a characteristic specimen as *Uintatherium robustum*, and likewise gave the name *Uintamastix*

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* As an example of the utility of this little apparatus, I may refer to Mr. R. H. Lee's paper on the atomic weights of cobalt and nickel. See this Journal, vol. ii, July, 1871.

† This Journal, vol. ii, p. 35.

‡ American Philosophical Soc., vol. xii, p. 420

atrox to an upper canine tooth, probably of the same animal, on the supposition that it pertained to a carnivore.*

The remarkable feature in the skull of this group was first indicated in the name *Tinoceras*, proposed by the writer (August 19th, 1872) for the genus represented by the type species, and subsequently mentioned in this Journal.†

Prof. Cope has since proposed the generic name *Eobasileus*,‡ and indicated three species, which apparently are not distinct from those previously described by Dr. Leidy and the writer. Many of the characters given by Prof. Cope in his description of these animals do not indeed apply to the other known species, but it is evident he has made several serious mistakes in his observations. Among the more important of these errors are the following:—What Prof. Cope has called the incisors are canines, and hence his statement that there are large incisor tusks, but no canines, should be reversed. 2d. The stout horns he described are not on the frontals, but on the maxillaries. 3d. The orbit is not below these horns, but quite behind them, and it has over it a prominent ridge on the frontal. 4th. The occiput is not vertical, but extends obliquely backward, the occipital crest projecting behind the condyles. 5th. The temporal fossæ are not small posteriorly, but unusually large. 6th. The great trochanter of the femur is recurved, although Prof. Cope says not. 7th. The spine of the tibia is not obtuse, but wanting. One of the species named by Prof. Cope (*Eobasileus furcatus*), is based on what he regards as portions of the nasal bones. The description, however, indicates that these specimens are merely the posterior horn cores of well known species.

The Museum of Yale College contains the remains of many individuals of the order *Dinocerata*, including the types of the various species described by the writer.§ All of these are well represented by characteristic specimens, and one species, *Dinoceras mirabilis* Marsh, by an entire skull, and a nearly perfect skeleton. An opportunity has thus been afforded of determining with some certainty the nature and affinities of this most singular group of animals, and the more important characters are here mentioned, preliminary to the full description. Most of the cranial characters are derived from a very perfect skull of *Dinoceras mirabilis*, figured in the accompaning plates.

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* Proceedings Philadelphia Academy, 1872, p. 169.

† Vol. iv, September, 1872, Erratum ; also October, 1872, p. 332.

‡ It is uncertain what date should be assigned to the name *Eobasileus*, and the species included under it by Prof. Cope. After a very careful investigation, I cannot ascertain that the descriptions were published before Oct. 29th, 1872, when copies were first received by the Philadelphia Academy of Nat. Science, of which Prof. Cope is secretary. The dates on the papers (Aug. 20th and 22d, 1872) certainly do not represent those of actual publication. The descriptions have not yet appeared in the Proceedings of the American Philosophical Society, where they were read (Sept. 20th, 1872), and hence no exact reference to them can at present be given. Several other papers by Prof. Cope on fossil vertebrates from Wyoming bear various dates from July 11th to October 12th, 1872, but apparently none of them were published before October 29th, and some of them certainly not until about a month later.

§ This Journal, vol. iv, pp. 322, 323, 343, Oct. 1872. Also Proc. Am. Philosophical Soc., vol. xii., Dec., 1872, and Am. Naturalist, vol. vii, p. 52, Jan., 1873.

The skull is unusually long and narrow. The three pairs of horn cores, rising successively above each other, and the huge crest around the deep concavity of the crown, together with the large decurved trenchant tusks, unite in giving a most remarkable appearance to the entire head (Plates I–II), which differs widely from anything known among living or fossil forms.

The structure of the skull presents many features of interest. The supraoccipital is greatly developed, and, after rising above the brain case, forms an enormous crest which projects obliquely backward beyond the condyles. This crest is continued forward on either side, each lateral portion sloping outward, and overhanging the large temporal fossa. This portion of the crest is formed largely of the parietals. The posterior pair of horns rise from this crest, which is thickened below on the inner side to support them. In front of these horns the crest descends rapidly, and subsides nearly over the center of the orbit. These posterior horn-cores are higher than those in front, and have obtuse summits, flattened transversely. (Plates I–II.) The frontal bones have no postorbital process, and the orbit is not separated from the temporal fossa. The latter is very large posteriorly. (Pl. II, fig. 1.) The squamosal forms the lower portion of the temporal fossa, and sends down a massive post-glenoid process. It likewise sends forward a zygomatic process, which resembles that of the Tapir. The malar completes the anterior portion of the arch, which is not the case with any known Proboscidian. The lachrymal is large, and forms the anterior border of the orbit, as in the Rhinoceros. It is perforated by a large foramen on its facial surface. Over the orbit, the frontal sends out laterally a prominent ridge, which afforded good protection to the eye in the combats of these animals with each other. On this ridge, there is a small protuberance, which resembles a diminutive horn-core, but its position, immediately in front of the lateral crest, indicates that it did not support a true horn.

The maxillaries are massive, and quite remarkable in supporting a pair of stout conical horn-cores. The bases of these cones approximate, and their summits are obtuse and nearly round. (Plates I–II.) Below these horns are the huge decurved canines, the extremity of the fang being implanted in the base of the horn-core. Behind the canine, there is a moderate diastema, followed by six small premolar and molar teeth. The crowns of the molars are formed of two transverse ridges, separated externally, and meeting at their inner extremities. The nasals are massive, and greatly prolonged anteriorly. In on the maxillaries were conical, much elongated, and undoubtedly formed most powerful means of defence. The posterior horns were the largest, and their flattened cores indicate that they were expanded, and perhaps branched. All the horn cores are solid, nearly smooth externally, and none of them show any indication of a burr. Whether both sexes had horns, cannot at present be decided, but this was probably the case.

The remains on which this description is based are all from the Eocene deposits of Wyoming. A more complete description, with full illustrations, is in course of preparation.

YALE COLLEGE, New Haven, Jan. 13th, 1873.

Postscript.

Since the above was in type, a short paper by Prof. Cope on the same subject, read before the Philadelphia Academy, and bearing the date of Jan. 16th, 1873, has been received. The paper contains no new points of importance, and is marred by many errors. The author aims to show that these animals are true *Proboscidea*, and possessed a proboscis. As I have already answered both these points fully, it is unnecessary to discuss them here. It is, however, important to promptly correct some of the more palpable mistakes in the paper, among which are the following: 1st. The genus *Dinoceras* was not originally referred to the Perissodactyls, but to a new order. 2d. The type species of this order was not described as *Titanotherium anceps*, but as *Titanotherium? anceps*, a difference of importance, as the reference was merely provisional, and the characters given pointed, not to the Perissodactyls, but to Proboscidians. 3d. The date given to *Eobasileus* (Aug. 20th, 1872) is not correct, as is stated above, page 118. 4th. The name *Tinoceras* was not first proposed Aug. 24, 1872, but Aug. 19, 1872, and on that day I mailed Prof. Cope the pamphlet containing it. 5th. The communication I made on this subject before the American Philosophical Society was not Dec. 30th, 1872, but Dec. 20th, 1872, Prof. Cope being present. 6th. The nasal bones in the *Dinocerata* are not exceedingly short, but much elongated. 7th. The malar bone does not form the middle element of the zygomatic arch, but the anterior, as in the Tapir. 8th. The frontals do not have a great prolongation forward, and it is very doubtful if they support horns or processes at both extremities. 9th. The nasal bones are not deeply excavated at their extremities. The assertion that it is "exceedingly probable that the tusk of the Mastodon and Elephant, regarded as an incisor by Cuvier, is really a canine," needs no refutation. If Prof. Cope will examine the skull of a young elephant, he will probably find that Cuvier was right after all.

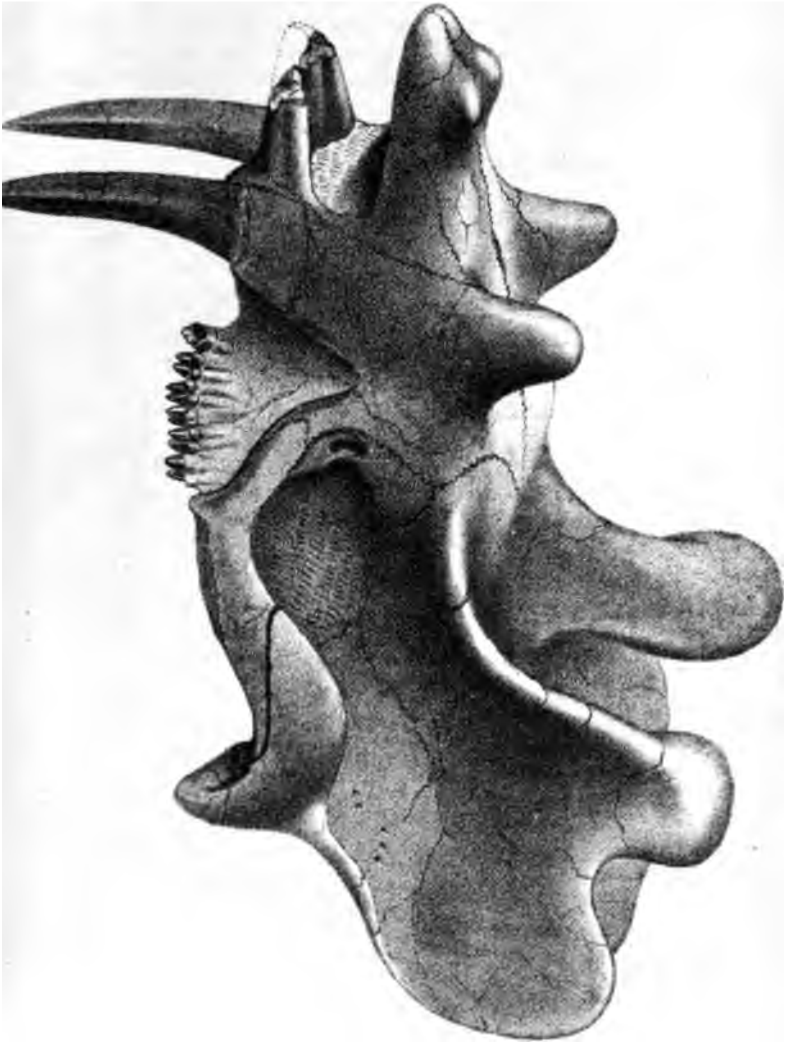
Yale College, Jan. 21st, 1873.

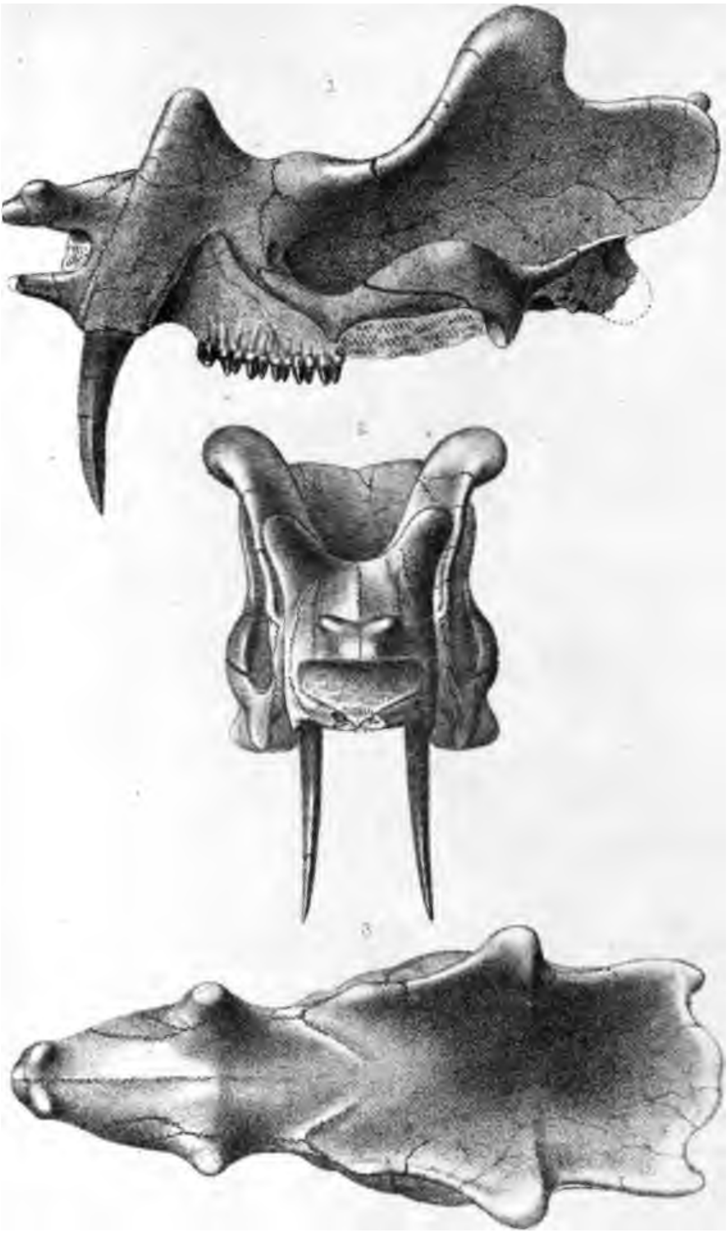
EXPLANATION OF PLATES.

ate I. *Dinoceras mirabilis* Marsh. Oblique view. One-fifth natural size.

ate II. *Dinoceras mirabilis* Marsh. Figure 1, side view; figure 2, front view; figure 3, top view. All one-eighth natural size.

PLATE I





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Punderson & Crisand New Haven

DINOCERAS MIRABILIS Marsh 18

[.—*On the Experimental Determination of the relative es of Sounds; and on the measurement of the powers of substances to Reflect and to Transmit Sonorous Vibra-*

y Alfred M. Mayer, Ph.D., Professor of Physics tevens Institute of Technology.

ɔre the National Academy of Sciences, in Cambridge, Nov., 1872.)

(Concluded from page 46.)

the resonators have such distances from their corres- ɔunding bodies that the phases of the impulses on ·ane are opposed while their intensities are different, action is given, and the intensity of this action on the will depend on the relative intensities of the sources and the relative distances at which the resonators

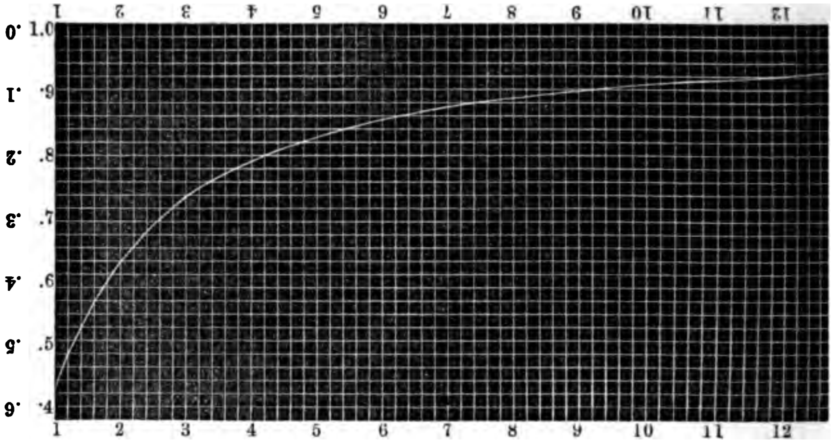
It may here be interesting to consider the simplest s, when the intensities of vibration at the two sources f the sounds are the same, and the two resonators are various distances from these points of origin, but fer in their distances by one half wave-length. Let ›ne of the resonators, B the other. Let A be succes- ed at distances from its sounding body equal to 1, 2, e-lengths, and B successively at distances equal to 1½, ., wave-lengths. When the resonators are in the tions we will suppose that the phases of vibration ıe membrane are opposed. The following table gives ıtions made on the assumption that the intensities ·ations diminish as the reciprocals of the squares of ıces from the sounding bodies:

B's dist. in λ.	Ratios of Intensities.	Residual Effects.
1·5	·444	·556
2·5	·640	·360
3·5	·734	·266
4·5	·790	·210
5·5	·826	·174
6·5	·854	·146
7·5	·871	·129
8·5	·885	·115
9·5	·897	·103
10·5	·907	·093
11·5	·914	·086
12·5	·921	·079
13·5	·927	·073
24·5	·959	·041
25·5	·961	·039

› projected these related numbers in the accompany- whose abscissas represent the distances of A from the source of sound, and whose ordinates gives the ratios of intensities between A, taken at the distances on the axis of abscissas, and B at distances from its sounding body always one half wave-length greater than A's distance from its sounding body. The formula of the curve is

$$y = \frac{x^2}{(x+\frac{1}{2})^2}.$$

If the curve be placed up side down, and referred to the corresponding numbers on the abscissas and ordinates (the latter being equal to unity minus the numbers at the corresponding points of the curve when in its first position), we have the graphical representation of the variation of the resultant intensities, contained in the fourth column of the table.



In the case of notes of different pitch, giving the same amplitude of swing to the aerial particles, the higher note will necessarily force the air to make its vibrations with a greater velocity, and the intensities will therefore not alone depend on the amplitudes of these vibrations but also on their velocities, and it has been deduced from established principles of dynamics that the apparent intensities of notes of different pitch will vary directly as the squares of the amplitudes, and inversely as the fourth power of the wave-length or periodic time. (See Mr. Bosanquet on the Relation between the Energy and Apparent Intensity of Sounds of different Pitch, L. E. & D. Phil. Mag., Nov., 1872). Hence the determination of the relative intensities of notes of different pitch becomes very complicated, and the experimental solution of the problem is encompassed with many difficulties. I however hope to be able, at some future day, to present some work in this direction when I have succeeded in obtaining results worthy of the appellation of measures of precision.

2. *Measurement of the powers of various substances to transmit and to reflect sonorous vibrations.*[*]

After we have succeeded in obtaining a measure of the intensity of the vibrations of the air at a certain distance from the sounding body, we can measure the poewrs of various substances to transmit, absorb and reflect sonorous vibrations.

To accomplish this I place one of the sounding bodies in the focus of a parabolic reflector and bring the two resonators at such distances from their sounding bodies that the intensities of the pulses traversing their respective tubes are equal. We then place in front of, but not too near, the mouth of the resonater, in front of the reflector, the plane surface of the substance whose transmitting and reflecting powers we would determine. Serrations now appear in the flame, because part of the force of the pulses which previously sounded the resonator are now reflected from the interposed substance. The resonator which has not the reflecting surface in front of it is now gradually drawn away from its sounding body, and at each successive point of remove the pulses propagated through the two resonator tubes are brought to opposition of phase on reaching the membrane by means of the glass telescoping tube. Equality of impulses having been obtained, we measure the distance of the resonator, which has not the reflecting substance in front of it, from the origin of its sounding body, and this measure, together with the known previous distance of this resonator, when equality was attained before the interposition of the reflecting surface, gives the data for the computation of the intensity of the *transmitted* vibration. This number substracted from the measure of the intensity when the substance was not before the resonator, taken as unity, gives the reflecting power of the substance plus its absorbing power.

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[*] In the Smithsonian Report for 1857 will be found an account of very interesting and valuable experiments, by Prof. Joseph Henry, bearing on "Acoustics applied to Public Buildings." In these investigations, Prof. Henry determined the *relative* powers of various substances to reflect, transmit and absorb sonorous vibrations by placing on the bodies the foot of a tuning-fork, and comparing the duration of its sound when thus placed with that given when the fork was suspended in the free air by a fine cambric thread. Thus suspended the fork vibrated during 252 seconds. Placed on a large, thin pine board, its vibrations lasted about 10 seconds. In this case "the shortness of duration was compensated for by the greater intensity of effect produced." The fork having been placed successively on a marble slab, a solid brick wall, and on a wall of lath and plaster, its vibrations lasted respectively 115 seconds, 88 seconds, and 18 seconds.

Placed on a cube of india-rubber, the sound emitted by the fork was scarcely greater than when it was suspended from the cambric thread, but its *duration* was only 40 seconds. Here Henry puts the question, what became of the impulses lost by the tuning-fork? They were neither transmitted through the india-rubber nor given off to the air in the form of sounds; but were probably expended in producing a change in the matter of the india-rubber, or were converted into heat, or both. Though the inquiry did not fall strictly within the line of this series of investigations, yet it was of so interesting a character in a physical point of view to determine whether heat was actually produced, that the following experiment was made. * * * The point of a compound wire formed of copper and iron was thrust into the substance of the rubber, while the other ends of the wire were connected with a delicate galvanometer. The needle was suffered to come to rest, the tuning-fork was then vibrated, and its impulses transmitted to the rubber. A very perceptible increase of temperature was the result. The needle moved through an arc of from one to two and a half degrees. The experiment was varied, and many times repeated; the motions of the needle were always in the same direction, namely, in that which was produced when the point of the compound wire was heated by momentary contact with the fingers." We have pleasure in again calling attention to this most beautiful experiment of Prof. Henry, for he was, I believe, the first to obtain the production of heat on the *absorption* (so to speak) of sonorous vibrations; and although several experimenters have subsequently obtained the same results, not one of them gives Henry credit for antecedent work. In 1868 I published a full account of the above experiment in my *Lecture, Notes on Physics*, p. 79. Van Nostrand, N. Y.

In the same paper Professor Henry obtained a few qualitative relations in the reflecting powers of various substances, by placing a watch between the centre and focus of a concave mirror; he then receded along the axis of the diverging sonorous beam, with a hearing trumpet. Paper and flannel were now stretched between the watch and the mirror, and the intensity of the sound was found to be diminished by the reflecting and absorbing powers of these substances.

It is very important, in such measures, to be sure that a plane wave surface is reflected from the mirror. This character of wave can be approximately obtained by placing the mouth of a closed organ-pipe at or very near the principle focus of the mirror and testing, by the method we have described above, the equality of intensity of the vibrating air in front of the mirror as we recede along its axis. We thus, by trial, at last succeed in obtaining a sufficiently plane wave-surface. Care must also be taken that the surface of the reflecting substance is so large that no inflected vibrations can act on the resonator.

I have made several measures of intensity and of transmitting and reflecting powers, but as the experiments were made in a room whose walls, ceiling and floor gave reflected sonorous waves, I will not present measures until I have arranged suitable apartments for their accurate execution.

November 13th, 1872.

ART. XVII.—*Meteoric Shower of November* 27–28, 1872, *as observed at the Observatory of Moncalieri* (*Italy*); by Padre DENZA. (From a letter addressed to Admiral SANDS, U. S. Naval Observatory, Washington, D. C.)*

A GREAT meteoric shower, the greatest hitherto observed in our country, was seen yesterday evening at this Observatory, and I am sure that it must have been observed likewise in many other places. It commenced with the dusk, and the meteors kept falling till after midnight, and must have continued still later, but a fog prevented us from following them longer.

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* Translated under the direction of Admiral Sands, Superintendent of the Observatory, and by him communicated to this Journal.

Thirty-three thousand four hundred (33,400) meteors were counted here in six hours, from half past six to half past twelve, by four observers. This number, moreover, only represents incompletely the real extent of the meteoric shower, because in the first hour of the night, and especially in the hour of the maximum fall, which was about eight o'clock, there was in some regions of the heavens truly a rain of fire similar in every respect to what is seen at the explosion of what are called *grenades* in artificial fireworks. It was indeed continuous, and the flocks of fire fell as if vertically in crowds, and like rain rather than thinly or moderately; so that it was impossible to note any but the most remarkable among them. At this time our observers counted about four hundred meteors each minute and a half.

All the wonderful and beautiful appearances which have been described in the grand meteoric showers of the 14th of November passed before us. Many meteors showed the most varied and delicate colors; many others were followed by broad and brilliant tracks of fire; very frequently balls of dazzling light, some with a diameter little less than that of the moon, were seen. Light and transparent clouds broke here and there in the atmosphere, splitting into belts of rays of the most fleecy and fantastic form. From time to time some of these clouds remained fixed in the celestial vault, and shone for some time, and there was one which appeared at 6^h 35^m P. M., between Perseus and Auriga, and did not disappear until 6^h 56^m, that is, twenty-one minutes later. In fact, the general aspect of the phenomenon was that of a cosmical cloud which, encountering our atmosphere, was broken in pieces and scattered. The position of the radiant, which I am accurately determining, and which is near γ Andromedæ, and the epoch of the apparition, induces me to believe that the cloud or meteoric-current which we have traversed is the same which is seen every year at this period of time, but with a greatly diminished intensity. It is the same which was seen by Brandes on the 7th of December, 1798, and afterward re-observed the same day in 1830 by the Abbé Raillard, and then in 1838 by Herrick and Flaugergues; more lately, in 1867, it was recognized at Bergamo by Zezioli. At the present its point of contact with the earth's orbit should fall precisely on the 27–28th November.

Now, as you are aware, from sufficiently probable calculations, it appears that this same meteoric current follows the orbit of the very remarkable comet of Biela, whose appearance was expected also this year in the month of October, and which has been sought for in vain, up to this time, by astronomers. Nothing is more probable, therefore, than that the great meteoric cloud which gave us the shower of yesterday, came from a portion of this body broken up and dissolved; especially if we consider that yesterday we passed through one of the two nodes of the orbit of the comet.

A beautiful aurora borealis was seen at Moncalieri at the same time from ten minutes past six to about eight o'clock P. M. The maximum took place at about seven o'clock, at which hour all the sky from north-northwest to northeast was tinted with a deep red color; afterward it remained very light and clear, especially from the west-southwest to the north. This phenomenon, moreover, is often the attendant of these great apparitions of falling stars, and gives rise to many hypotheses and conjectures. This aurora was likewise seen at Perugia, Messina and other places. P. F. DENZA.

P. S. I have received accounts from Turin, Bra, Dogliani and Mondovi in Piedmont, as well as from P. Secchi at Rome and from the Prince of Lampedusa at Palermo; from Professor De Gasparis at Naples, from Prof. Bellucci at Perugia, from Prof. Eugenio at Matera, from Prof. Lachianca at Messina. All attest the grandeur of the apparitions seen in those places.

At Naples Prof. De Gasparis counted about two meteors every second; at Matera, in the southern provinces, Prof. Vito Eugenio, with three assistants, counted 38,153 meteors in six hours, that is, from six to twelve o'clock. At Messina they were not able to count the falling stars, so great was their multitude. At Mondovi, Prof. Bruno, with three assistants, counted 30,881 meteors from 6^h 18^m P. M. to 2^h 15^m A. M. The maximum took place everywhere between 8 and 9 o'clock, and the radiant has been found to be not far from γ Andromedæ. The shower of Nov. 14th would have been observed at a great number of Italian stations, but in most of them the observers experienced bad weather. In some of the stations the apparition was very poor. At Matera alone were satisfactory observations made.

The following numbers of stars were observed at Matera in the successive half hours from midnight to six o'clock A. M. of Nov. 14th, viz.: 7, 10, 9, 13, 17, 25, 41, 79, 122, 149, 109 and 57. Total 638.

XVIII.—*Experiments for the determination of the height to ich liquids may be heaped above the edge of a vessel;* by C. Mendenhall, Columbus, Ohio.

E most careless observer is aware of the fact, that if any d is poured quietly into a vessel, it may be heaped up until essel holds more than its actual capacity. A few weeks since ttention was called to this fact in such a manner as to make m worth while to devote a little time to the study of this known phenomenon. I have accordingly made a considera- eries of experiments upon the heaping of various liquids in us vessels, the results of which are not without interest. necessity is at once felt for some device for getting the liquid e vessel other than the method of dropping, which of course rbs the heaped liquid, and causes it to overflow much sooner it otherwise would. I therefore devised the following appara- which gave very satisfactory results. The vessels, which were ly of glass, and which varied in diameter from one-fourth of ch to two and a half inches, were arranged with an opening e bottom through which the liquid could be forced. This ing was attached by means of a flexible tube to another larger l, into which the liquid was poured, which was so arranged a slow motion in a vertical direction could be given it, under control of a screw movement, so that it could be raised or red at will and through an inappreciable distance if desired. e metallic point was made to move vertically over the center e vessel in which the liquid was to be heaped, being attached vernier scale reading to thousandths of an inch. The man- f making a measurement was as follows:

e vessel with which the trial was to be made was first care- leveled, being for that purpose supported upon a tripod stand leveling screws. The metallic point, after having been ad- d as near as may be over the center, was brought down to evel of the rim of the vessel, this being determined by plac- straight bar across the center, and the vernier was then read. er, if the trial was made with water, was then poured into other vessel until it reached the top or near the top of the vessel. The slow movement of the screw raised the one, gradually, through the connecting flexible tube, conveyed water into the other, forming a beautiful heap above its peri- r. A screw movement carried the metallic point just in nce of the rising column of water, contact with it, which be prevented in most cases if accurate results are sought for, g very delicately avoided by viewing it in relation to its e reflected from the surface of the liquid. This being con- d until overflow begins, the vernier is again read, the differ- of the reading giving, of course, the height to which the d is heaped above the perimeter of the vessel. In order to mine the influence, if any, of the diameter of the vessel, a series of measurements was made with seven cups of various diameters, the liquid used being pure water at the temperature of the room, which was about 70° F. The results are collected in a table below.

No. of vessel.	Diameter in inches.	Height of water heaped in inches.
1	2·57	·196
2	1·91	·182
3	1·28	·190
4	·7	·203
5	·42	·182
6	·47	·154
7	·25	·148

In spite of the small differences in the results, I am inclined to the opinion that they justify the conclusion that, within certain limits cutting off cups of very small diameter, the height to which the liquid may be heaped is independent of the diameter of the vessel. I found the origin of these differences in the character of the edge of the vessel. The edges of Nos. 2 and 5 had been slightly rounded by fusion; Nos. 6 and 7 decidedly so: while No. 4 had the most even and perfectly ground edge of all. The thickness of the edges differed greatly, which seemed to show that the height reached was also independent of that. The edges measured in thickness as follows:

No.	Thickness of edge in inches.	No.	Thickness of edge in inches.
1	·056	4	·045
2	·048	5	·013
3	·149	6	·068
		7	·025

In order to test the effect of the thickness of the edge, to the top of No. 3 was sealed a circular glass plate, about two inches in diameter, having a circular opening about two-tenths of an inch in diameter in the center, thus forming, in effect, a vessel with an edge nine-tenths of an inch broad. The result of several trials with this entirely verified the previous conclusion. No. 4 being the most perfect cup of all, was selected for the trial of one or two other liquids and also to determine the effect of temperature upon the height to which water might be heaped. The result in the last case was precisely what might have been expected, as is clearly shown in the following three trials:

Water,	70° F.	·203 inches.
"	88° F.	·180 "
"	135° F.	·130 "

The difficulty of maintaining, during the measurement, a temperature higher than the last given above, prevented me, at the time, from making any further trials, but the results would, without doubt, verify the law indicated above.

[illegible] the same cup, mercury at a temperature of 70° F., was heaped [illegible] height of ·140 inches; alcohol to a height of ·094. In a brass with a diameter of about half an inch, water was heaped to a ht of ·176 inches. The edge of No. 4 was rubbed with tallow; r at a temperature of 75° F. was then heaped to a height of inches. Judging from these results, it seems that the height hich a liquid may be heaped depends upon the nature and lition of the liquid, and is, to a great extent, independent of ature and size of the vessel. In all of these trials the edge of essel was circular; what would be the effect of a modification is form I have not yet determined.

SCIENTIFIC INTELLIGENCE.

I. CHEMISTRY AND PHYSICS.

On the spectrum of nitrogen.—SCHUSTER has studied the trum of nitrogen gas under different pressures, and in a con- n of chemical purity not hitherto obtained in such experi- ts. The results of this investigation are of much interest and e. Plücker and Hittorf in 1865 endeavored to show that one the same gas is capable of giving different spectra. In the of nitrogen, Secchi obtained with the same tube three differ- pectra. Schuster, in repeating this experiment, obtained only gle spectrum of lines, corresponding to Plücker's spectrum e second order. But this spectrum suddenly changed, and well-known cannellated bands appeared. The introduction of yden jar brought back the line spectrum, but after a short the discharge no longer passed through the tube. The author equently suspected that the bands were due to the presence ir, and this proved to be the case. To eliminate oxygen com- ly from the tube, Schuster heated small pieces of sodium in gas. After this treatment, the line spectrum only could be ined. The author measured the wave-lengths of the cannel- l bands as well as those of the nitrogen lines, with the follow- esults:

	Bands.	
512·9	455·6	431·8
498·1	443·6	423·7
464·9	439·0	
	Nitrogen lines.	
628·8	566·6	418·4
616·5	516·4	
615·2	489·4	
594·2	464·4	
593·2	421·4	

true spectrum of nitrogen is characterized by an intense line. Toward the violet there is a green, not shaded, band, there are also a few unshaded violet lines. Pure nitrogen ited only one spectrum, whatever might be the pressure.

The author also studied the spectra of the different oxides of nitrogen, but found that these gave only the cannellated spectrum hitherto attributed to nitrogen, and which undoubtedly belongs only to the oxide of nitrogen formed under the influence of the electric spark. Schuster's results clear away an extraordinary amount of confusion and error. It is to be hoped that they will lead to a careful revision of the spectra of the other simple gases. —*Pogg. Ann.*, cxlvii, p. 106. W. G.

2. *On new modes of forming amides and nitriles.*—At the suggestion of A. W. Hofmann, LETTS has studied the action of certain fatty and aromatic acids upon potassic sulpho-cyanide. Powdered sulpho-cyanide dissolves readily in boiling acetic acid, with evolution of carbonic dioxide and sulphydric acid. After boiling for some days, the mixture was distilled. What passed over at 216°–220° C. solidified to a mass of acetamide, and potassic acetate remained in the retort. The reaction in this case is represented mainly by the equation:

$$CHNS + C_2H_3O\,.\,OH = \left.\begin{matrix}C_2H_3O\\H_2\end{matrix}\right\}N + COS.$$

The oxysulphide of carbon set free was easily recognized by direct experiment, but the sulphydric acid and carbonic dioxide are due to a secondary reaction expressed by the equation:

$$\left.\begin{matrix}C_2H_3O\\H_2\end{matrix}\right\}N + COS = C_2H_3N + CO_2 + H_2S.$$

Potassic sulpho-cyanide with iso-butyric acid yields iso-butyramide and iso-butyro-nitril, $\left.\begin{matrix}C_4H_7O\\H_2\end{matrix}\right\}N$, and C_4H_7N. Valeric acid yielded precisely similar results. The action of benzoic acid upon potassic sulpho-cyanide resulted in the almost exclusive formation of benzonitril. In like manner cuminic acid gave cumo-nitril, but cinnamic acid appeared to be decomposed into carbonic acid and cinnamol before it exerted any action on the sulpho-cyanhydric acid.—*Berichte der Deutschen Chem. Gesell., Jahrgang* v, p. 669. W. G.

3. *On bibromide of terpene.*—BIEDERMANN and OPPENHEIM have studied the action of oxidizing agents upon bibromide of terpene. A mixture of potassic chromate and sulphuric acid was found most advantageous, and yielded terephthalic acid, $C_8H_6O_4$, and a small quantity of mono-bromhydrate of terpentine, $C_{10}H_{17}Br$. In this reaction it is probable that cymol is first formed by the separation of bromhydric acid, and that this is then oxidized to terephthalic acid. The result is of importance as another proof of the connection between the terpentine and benzol series.—*Ibid*, p. 627. W. G.

4. *On cymol from oil of terpentine and oil of lemons.*—OPPENHEIM has also studied the cymols obtained from terpen-bibromide and citren-bibromide by heating them with anilin in sealed tubes. Both yielded terephthalic and acetic acids; whence it follows that the two cymols are identical. Oil of terpentine and oil of lemons appear, therefore, to be hydrogen compounds of the same cymol,

which differ only in the relative position of the two atoms of hydrogen which unite with the cymol. In examining this subject farther, the author distilled off portions of unoxidized cymol from both oil of terpentine and oil of lemons, and oxidized these a second time. After twenty grams of terpen-cymol had been completely oxidized, the condensing tube contained about a gram and a half of a white crystalline body, having the composition and properties of camphor.—*Ibid*, pp. 628 and 631. W. G.

5. *On the combinations of yttrium and erbium.*—Cleve and Hoeglund have published the first part of an examination of the compounds of yttrium and erbium. The authors employed the method of separation and purification devised by Bahr and Bunsen, and with these chemists deny the existence of terbium, and consider the earths in gadolinite to be oxides of yttrium, erbium and didymium. A re-determination of the atomic weights of yttrium and erbium gave for the former 59·7, and for the latter 113·7; the authors did not succeed in isolating either metal, either by the electrolysis of the fused chlorides, or by reducing the chlorides with sodium. Oxide of yttrium obtained by heating the hydrate to whiteness forms pieces of a yellowish-white color, easily soluble in acids; its density is 5·03. When heated for some days in a porcelain furnace with a little borax, it crystallizes in transparent, microscopic prisms. The hydrate is a white, gelatinous precipitate, which absorbs carbonic acid readily from the air, and decomposes ammoniacal salts. Erbia prepared from the hydrate presents hard pieces of a dirty rose color, and density 8·8 to 8·9. It dissolves with difficulty even in strong acids, but the oxalate gives by heating a yellowish-red powder, which dissolves easily in acids. The hydrate, $2(ErH_2\Theta_2)+\Theta H_2$, is rose-colored. The salts of yttria are colorless, and give no absorption spectrum; they are in general less soluble than the corresponding salts of erbium. The formulas of the salts examined are as follows:

$YCl_2+4\Theta H_2$ $YBr_2+6\Theta H_2$ YF_2 $Fe_2Cy_{12}Y_3K_2+4OH_2$
$[Co_2Cy_{12}Y_3+5\Theta H_2$ (?)
$ErCl_2+4\Theta H_2$ $ErBr_2+6OH_2$ ErF_2 $Fe_2Cy_{12}Er_3K_2+x\Theta H_2$
$[Co_2Cy_{12}Er_3+5\Theta H_2$
$Y(CyS)_2+4\Theta H_2$ $PtCy_4Y+7\Theta H_2$ $Y(N\Theta_3)_2+4\Theta H_2$
$[Y(Cl\Theta_3)_2+6\Theta H_2$
$Er(CyS)_2+4\Theta H_2$ $PtCy_4Er+7\Theta H_2$ $Er(N\Theta_3)_2+4\Theta H_2$
$[Er(Cl\Theta_3)_2+6\Theta H_2$
$Y(Br\Theta_3)_2+6\Theta H_2$ $Y(I\Theta_3)_2+2\Theta H_2$ $Y_8I_6\Theta_{29}+24\Theta H_2$
$[3YS\Theta_4+8\Theta H_2$
$Er(Br\Theta_3)_2+6\Theta H_2$ $Er(I\Theta_3)_2+2\Theta H_2$ $Er_8I_6\Theta_{29}+24\Theta H_2$
$[3ErS\Theta_4+8\Theta H_2$

Of these salts the sulphates are probably most interesting; they correspond in composition to the sulphates of didymium and cadmium. The four sulphates have nearly the same atomic volumes. Sulphate of yttrium forms small transparent crystals, which dissolve easily in a saturated solution of potassic sulphate; but the solution on heating deposits an abundant precipitate of a double salt. Erbic sulphate yields larger crystals of a fine rose color, which like the sulphate of yttrium and of the cerium metals are less soluble in hot than in cold water.—*Bulletin de la Société Chimique*, xviii, p. 193. W. G.

6. *On the transformation of right tartaric acid into racemic acid.*—Pasteur's classical investigation of the four different modifications of tartaric acid are familiar to all chemists. JUNGFLEISCH has now succeeded in finding a simple method of converting dextrorsal into sinistrorsal acid, which does not require the use of an expensive reagent like cinchonicine, and which may be used with comparatively large quantities of material. The process in question consists in heating ordinary tartaric acid with a little water, in sealed tubes, to 175° C., for thirty hours. On carefully opening the tubes, a large quantity of gas escapes; the contents of the tube are then poured into water, and the solution filtered and evaporated. On cooling, racemic acid crystallizes, and may be purified by a second crystallization. The mother liquor from the crystals contains some unaltered tartaric acid, some inactive acid, and a little of the products of decomposition. The temperature must be kept exactly at 175°; in this case the transformation is almost complete. The racemic acid obtained in this way is identical with that obtained from urine. When converted into racemate of sodium and ammonium, it yields right and left hemihedral crystals. The author has found that the acid may be prepared on a comparatively large scale, by employing enamelled steel or iron vessels. In this manner 650 grams of tartaric acid and 120 grams of water were treated successfully in one operation. —*Bulletin de la Société Chimique*, xviii, p. 201.

W. G.

7. *On the conversion of anilin into toluidin.*—A. W. HOFMANN has succeeded in converting anilin into toluidin by the following process. Anilin is first converted into methyl-anilin by means of iodide of methyl. The salts of methyl-anilin—the experiments were made with the chlorhydrate and iodhydrate—may be heated for hours to 220°–230° C. without change, but if the temperature is raised to that of the melting point of lead (335°), the methyl is transferred from the amide residue to the benzol residue, or in other words, the methyl-anilin is converted into toluidin:

$$C_6H_5 . CH_3 . HN . HCl = (C_6H_4 . CH_3)H_2N , HCl.$$

The toluidin obtained in this way, after recrystallization, fused at 45° C. It is remarkable, however, that iodhydrate of methylanilin when treated in the same way yields a fluid toluidin, but the author leaves for the present undecided which of the fluid toluidines is formed. As results of his not yet completed investigation, Hofmann believes that he has obtained a base having the formula $[C_6(CH_3)_5]H_2N$, and a hydrocarbon with the formula $C_6(CH_3)_6$.—*Berichte der Deutschen Chem. Gesellschaft, Jahrgang* v, p. 720. W. G.

8. *Triethylmethane.*—LADENDURG finds that the so-called orthoformic ether $CH(OC_2H_5)_3$, when distilled with zinc-ethyl and sodium, yields two products, one of which appears to be an aldehyde but was not fully identified, while the other has the composition C_7H_{16}, and from the mode of its formation is doubtless *triethylmethane*, $CH\,(C_2H_5)_3$. This heptane is a colorless liquid having a faint petroleum odor and boils at 96° C.—*Ber. D. Ch. Ges.*, v, 752. S. W. J.

9. *Triphenylmethane.*—KEKULÉ and FRANCHIMONT remind that the aromatic hydrocarbons may not only be regarded as derivatives of benzol, with more or less hydrogen replaced by univalent alcohol radicals, as is the present habit; but may likewise be formulated as fatty hydrocarbons containing benzol residues. Marsh gas would thus give—

$CH_3.C_6H_5$	Phenylmethane.
$CH_2.(C_6H_5)_2$	Diphenylmethane.
$CH.(C_6H_5)_3$	Triphenylmethane.
$C.(C_6H_5)_4$	Tetraphenylmethane.

The first of these is toluol, commonly regarded as methyl-benzol. The second is the substance recently investigated by Zincke, and termed by him benzyl-benzol. The third the authors have obtained by heating together to 150–155° C. a mixture of 1 molecule benzylenechloride $C_6H_5\,.\,CHCl_2$, and 2 molecules of Otto's mercury-diphenyl.

Triphenylmethane is a crystalline solid fusing at 92·5° and boiling at about 355°. From its benzol solution, it crystallizes in combination with a molecule of the latter, and the crystals on exposure lose the benzol, becoming opaque and leave finally the pure triphenylmethane as easily powdered pseudomorphs.—*Ber. D. Ch. Ges.*, v, 906. S. W. J.

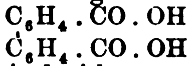
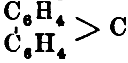
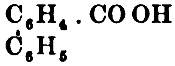
10. *Constitution of Ultramarine.*—UNGER, recalling an observation of Berzelius that lapis lazuli dissolves in melted salt of phosphorus to a clear bead with continuous effervescence, finds that ultramarine contains a considerable proportion of nitrogen. A sample of artificial ultramarine had the following composition:

Sulphur,	12·6	Aluminium,	14·4
Nitrogen,	5·5	Silicon,	20·4
Sodium,	14·1	Oxygen,	33·

It contained, however, no sodium sulphide and no acid of sulphur. Unger gives his reasons for supposing that the blue substance is $Al\,Si\,S_2N_2O_3$. The slightly blue-green mass obtained by igniting together, $Al\,O_3+Si\,O_2+4Na_2S_2O_3+2Na_2CO_3$ and washing with water which removed $4Na_2SO_4$ nearly, became intensely blue on igniting with salammoniac whereby NaCl was eliminated and hydrogen escaped partly as such, and partly as water, while nitrogen entered into the composition of the ultramarine. In the common preparation of this pigment the nitrogen is derived from the atmospheric air.—*Ber. D. Ch. Ges.*, v, 893. S. W. J.

11. *New Coal-tar Hydrocarbons.*—Fittig read before the German Naturalists' Association at Leipzig last summer, a brief notice of a new coal-tar hydrocarbon, which he then considered to have the composition $O_{16}H_{12}$, and to be phenyl-naphthalin, $C_6H_5C_{10}H_7$, which fused at 98-99°, and had a higher boiling point than anthracene.

On Nov. 1, Graebe communicated to the Berlin Chem. Society, a short paper on an isomere of anthracene, which Dr. Glaser had obtained nearly pure, in working anthracene on a large scale. The substance is described by Graebe as similar in aspect to anthracene, crystallizing in plates, and exhibiting blue fluorescence. It fuses at 105° and boils at 340° C. It sublimes less easily than anthracene and offers greater resistance to oxidizing agents. Its quinone, obtained by aid of chromic acid, forms yellow needles darker in color than ordinary anthraquinone, and the fusing point is 205°, or 68° lower than that of the latter.

On Nov. 19, Ostermayer and Fittig sent to the Berlin Chem. Society a paper in which they give, as the result of further investigation, that the hydrocarbon of Fittig is an isomere of anthracene, and the same which Graebe had received from Glaser. But as the points of fusion of the hydrocarbon, 97-99° and of its quinone 198°, are 6-8° lower than Graebe found, it is perhaps possible that the results belong to two distinct substances. Ostermayer and Fittig obtained by treating their quinone with chromic acid, a new dicarbon acid  $\begin{matrix} C_6H_4 . CO . OH \\ | \\ C_6H_4 . CO . OH \end{matrix}$ which they term diphenic acid. This acid ignited with excess of quick lime yields diphenylene-ketone,  $\begin{matrix} C_6H_4 \\ | \\ C_6H_4 \end{matrix} > CO$ which on fusion with potassium hydroxide, gives the acid  $\begin{matrix} C_6H_4 . CO OH \\ | \\ C_6H_5 \end{matrix}$

Ostermayer and Fittig conclude from these reactions that their hydrocarbon has the constitution assigned by Graebe and Liebermann to common anthracene.

E. Schmidt, Nov. 18, communicated to the Berlin Society a short account of another isomere of anthracene obtained by reducing the red mononitro derivative of ordinary anthracene, with tin and hydrochloric acid. The substance considerably resembles ordinary anthracene, forms thin brilliant plates, manifests blue-violet fluorescence and fuses at 247°, or 34° higher than on anthracene. This isomere also appears when the red mononitro-anthracene is heated to sublimation between watch-glasses, together with a yellow substance probably dinitro-anthracene. S. W. J.

12. *New Platform Balance.*—Messrs. Becker & Sons have recently constructed a new platform balance which has so many advantages for laboratory as well as for druggists' use, that a brief notice of it may be serviceable to the public. It consists of a low rectangular box of mahogany, containing the mechanism, very simply and durably constructed, and through ½ inch holes in

the top of this box pass the two sockets which support the pans. The latter are six or more inches in diameter, movable and nickel plated. The jointed beam and its bearings are made with knife edges and stirrups as in the analytical balances, and are quite thoroughly protected from dust and fumes by the tight case. A balance of this kind belonging to the Sheffield Laboratory, when loaded with 100 Troy ozs. in each pan, is sensitive to ½ grain. It is therefore both stout and delicate enough for standarding liter flasks, aliquoting bulky solutions, or for other large weighings, where considerable accuracy is required. Becker & Sons make several sizes; the one referred to is the smallest, and costs $32.

S. W. J.

II. GEOLOGY AND NATURAL HISTORY.

1. *On Tin discoveries in Queensland. Report by* Mr. T. F. GREGORY, Mineral Land Commissioner, dated Stanthorpe, July 2. —The general geographical area of the stanniferous country within the colony of Queensland, so far as is at present known, appears to be comprised within the following limits:—Commencing on the main dividing range between the eastern and western waters at Lucky-valley goldfields, near the head of the Condamine river, the northern boundary extends in a west-south westerly direction for about 25 miles, passing 15 miles south of the town of Warwick to the head of Pike's creek, on the Pikedale run; from this point it is bounded by a slightly curved line extending south about 20 miles to the Severn river, three miles below the Ballandean Head station, where it trends southeast for 12 miles further, meeting the boundary of New South Wales at the Tenterfield run; thence the crest of the Watershed, which forms the boundary between the two colonies, embraces it in a northeasterly and easterly direction back to Lucky-valley; the area comprised being, in round numbers, 550 square miles in extent. Of this area, however, only about 225 square miles have hitherto been found sufficiently rich in tin ore to pay for working, and, consequently, it is to this latter portion that my attention has been especially directed, although there are several instances of tin being found in payable quantities beyond these limits. The physical and geological character of nearly the whole of the area described is that of an elevated granitic table-land intersected by ranges of abrupt hills, the highest limits of which are about 3,000 feet above the sea, its eastern escarpment forming the watershed of the Clarence river, the northern that of the Condamine, and the southwestern the Severn and M'Intyre rivers. The passes and gorges whence these rivers issue from the elevated granitic country are mostly very steep and rugged, and difficult to traverse, especially to the north and eastward. Of the rivers and watercourses which intersect this tract of country, it will be only necessary for present purposes to refer to the Severn and its tributaries, as it is on them that by far the greater portion of mineral wealth

is found. The principal head of the Severn is the stream known as Quart Pot Creek, Four Mile, Law's, Ten Mile, Sugar Loaf and Thirteen Mile creeks drain the eastern portion of the district, comprising fully one-half of the country at present occupied by selections; while the northern head of the Severn, better known as the Broadwater, has for its tributaries Spring Creek, Reeve's Gully, Hardy's, and Cannon Creeks. Again, to the westward several watercourses, known as the 10, 13, 15, and 20 mile branches of Picke's Creek, flow westerly into a metamorphic formation in about seven or eight miles, when the tin-bearing country terminates. The majority of these watercourses rise in open sandy or rocky hollows, or shallower valleys, having at their commencement very little fall in them, and in ordinary seasons are well supplied with water, several being all but permanent streams; the main channels containing numerous large pools and sheets of water, in some instances over a quarter of a mile in length. The fall of the country from their sources to Ballandean Head station, an average distance of twenty miles, may be roughly estimated at from 400 to 600 feet, and in a few instances fully 1,000 feet. The portion of the district over which the principal deposits of tin ore are distributed is that comprised by the watershed of the Severn river down to Ballandean station, with the exception of about six miles of the extreme southern head of Quart-pot Creek and Accommodation Creek, both of which hitherto have not been found to yield payable ore. The richest deposits have been found in the stream beds and fluviatile flats on their banks, the payable ground varying from a few yards to five chains in width, occasionally broken by rocky bars, but even in these instances large deposits are frequently lodged in the pockets and crevices between the granite boulders. The aggregate length of these alluvial bands may be taken at about 140 miles on the Severn waters, with about 30 more on the tributaries to Pike's Creek. A very careful inquiry and personal examination of a number of the various workings that have been commenced within the last few weeks establishes very fair data upon which to estimate the probable yield of ore. This may now, with a tolerable degree of certainty, be stated at an average of 10 tons per lineal chain of the Creek beds. In some instances this has been found to extend to 30 tons per chain; but, allowing for frequent interruptions by rocky bars, it will be safer to adopt the first-mentioned yield as a fair standard upon which to base an estimate of the amount of mineral that it is probable will be raised within the next few years from alluvial working alone.

Of the stanniferous lodes or veins it is impossible at present to speak with any degree of certainty. The two principal ones as yet discovered are near Ballandean Head station and at an outlying reef of red granite rising up in the midst of metamorphic slate and sandstone at a spot known as the 'Red Rock,' and situated about six miles apart in a north and south direction; the other crosses the Severn several times at the point where the tin was first discovered and the land selected by Messrs. Greenup and others.

These lodes or veins have as yet been but very partially tested; t would, therefore, be premature to give any decided opinion on hem; they may, however, prove the source of an amount of wealth he production of which would extend over many years. There re also a number of smaller lodes or veins, some of which I have ot been able personally to inspect; the most promising appear to e on Law's Gully, on the claims of the Blue Mountain mining ompany, as well as on Quart Pot and Sugar Loaf Creeks; they un in parallel lines, bearing about north, 50 deg. east, commencing from near the boundary of New South Wales, opposite the Ruby Creek diggings in that colony, and again near the Broadwater, at a spot about a mile southeast from the junction of Hardy's Gully. This lode can be traced with interruptions all the way to he head of Spring Creek, a distance of nine or ten miles. In describing the mineralogical character of the rocks generally, I annot do better justice to the subject than by quoting from some valuable notes kindly furnished me by our well-known and talented geologist, Mr. D'Oyley Aplin, whose views on this subject oincide with my own observations. He says:—I have met with o other description of tin ore than peroxide (*cassiterite*), even in pecimens from veins. The ore, so far as I have seen it, is associated with granite only, which is invariably red, i. e., the feldspar s a pink or red orthoclase and the mica is generally black, but when crystals of tin ore are found *in situ* the mica is white. The granite, generally, is coarse grained, and seems to disintegrate eadily under atmospheric influence. There are numerous bands of loosely aggregated rock, granitoid in character, highly micaceous, and traversed by bands and veins of quartz in all directions; n these bands crystals of tin ore are abundant, and they (the bands) seem to have constituted local feeders along the courses of drainage. The crystals of tin ore are generally found embedded n and along the margin of the quartz threads or veins in those ands. In some instances they are embedded in the micaceous portions only, and the mica is invariably white in those instances, in bands referred to. The strike of the bands and the distinct quartz veins in the granite is generally northeast and southwest. Along he western margin of the granite a broad belt of metamorphic ocks (slates and sandstones) extends on both banks of the Severn, onstituting a series of rugged broken hills and ranges, in parts difficult to traverse except on foot; this tract of country stretches rom five to six miles west of Ballandean to Maidenhead-on-the-Severn, where the granite again appears and also the tin ore. No in floors, as at the Elsmore mine, in New South Wales, have yet een discovered.

2. *On Devonian Fossils in the Wahsatch Mountains;* by Prof. SANBORNE TENNEY, of Williams College. (Communicated to this ournal.)—Last August, while making some examination of the ocks in the Wahsatch Mountains, in a position southeast of Great Salt Lake City, I searched for fossils that would throw light on he age of this magnificent mountain range; and after a little

searching I found a locality where fossil corals are very numerous, and in quite a good state of preservation. This locality is on the divide between Great Cottonwood and Little Cottonwood cañons, and is, perhaps, nine thousand or ten thousand feet above the sea.

The rocks containing the fossils are a dark bluish limestone, and like the rocks of the region generally have a high northeasterly dip. The fossils were so marked that I had little hesitation in referring them to the Devonian. But that I might be sure as to their true place in the geological scale, I showed them to R. P. Whitfield, Esq., the distinguished paleontologist, who confirmed the opinion I had formed concerning their age, and referred the corals to *Zaphrentis* and to *Syringopora*. Of the latter there is only one species, perhaps, and this one Mr. Whitfield regards as closely allied to *S. Maclurii* Billings, but yet probably a distinct species, being considerably stronger or stouter. Of the former, namely, *Zaphrentis*, there are, at least, two species.

If these are genuine Devonian fossils—of the Upper Helderberg, according to Mr. Whitfield—they are the first of this period, so far as I am informed, that have been brought forward from the range of the Wahsatch.

Williamstown, Mass., Nov. 14, 1872.

3. *The Eruption of Vesuvius in* 1872, by Professor LUIGI PALMIERI, of the University of Naples, Director of the Vesuvian Observatory; with Notes, and an Introductory Sketch of the present state of knowledge of Terrestrial Vulcanicity, the Cosmical Nature and Relations of Volcanoes and Earthquakes; by ROBERT MALLET, Mem. Inst. C.E., F.R.S., &c. 148 pp. 8vo, with illustrations. London: (Asher & Co.) 1873.—On page 459 of the last volume of this Journal, an abstract is cited of an elaborate paper by Robert Mallet, C.E., on the sources of volcanic heat and energy, now in course of publication by the Royal Society. In the volume he has just published, whose title is given above, Mr. Mallet in addition to his translation of the very valuable memoir of Palmieri, presents in an introduction of 78 pages a more detailed account of his own views. His paper is the most important contribution to this department of geological dynamics which has ever been brought forward; and the work above mentioned is by his share in it more than doubled in value.*

4. *Geological Chart of Sweden.*—Numbers (Livraisons) 42–45 of the Geological Chart of Sweden, prepared by the Geological Survey of the kingdom—now under the direction of Otto Torell—were published during the year past. Like those preceding them, they are most beautiful specimens of engraving and printing in colors, exceeding any thing we have seen from other geological surveys.

5. *Kokscharow's Materialen zur Mineralogie Russlands.*—The first 96 pages of the 6th volume of this excellent work on Russian

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* Prof. Le Conte's recent paper in this Journal accords in some important respects with the views of Mr. Mallet. But the latter has the priority when there is agreement.

ineralogy, especially its crystallography, was issued in 1870. It eats of Olivine, Humite, Cerusite, with additions to the former otice, of Diamonds, Monazite, Humite, and Pyroxene.

6. *On the Origin and Characters of Officinal Rhubarb;* by H. AILLON.—Linnæus was acquainted with five species of *Rheum:* R. *Ribes*, *R. Rhaponticum*, *R. compactum*, *R. palmatum*, and *R. habarbarum*, which he named afterward *R. undulatum*. Since the ear 1762, *R. palmatum* has been generally considered the source of ficinal rhubarb. It was, however, known that during his Asiatic avels, Pallas showed the Bourbaskis the leaves of *R. palmatum*, nd that they declared that the leaves were unknown to them, and at those of the true rhubarb were round, and with numerous cisions at their edges. Guibourt, nevertheless, believed that is species was the source of the true Asiatic rhubarb of comerce, from the fact that among the kinds cultivated by him at aris, the root of *R. palmatum* had exactly the color and taste f Chinese rhubarb, although it did not crepitate under the teeth. lanchon, on the other hand, having studied histologically the agments in the Guibourt collection upon which this determination had been based, observed that they exhibited none of the natomical characters of Chinese rhubarb. The root-structure of R. *Emodi* and other Northern Indian species were known to gree no better. The medicinal product is derived therefore om a species with leaves lobed and palmately nerved, as in *R. ybridum* and *R. palmatum;* but the last has, as described by innæus, acuminate and palmate—not orbicular leaves. We may mark, however, that the true rhubarb of China and Russia does ot grow as Linnæus supposed, "ad murum Chinæ," but much ore to the west; and that, according to the *Punt-San* of the hinese, its axis resembles that of the Chinese Yam; it is moist, ntains a deep yellow substance, and is covered with dark bark. he leaves are also green in the first month (while those of *R. lmatum* are completely bleached), and well developed, the size f a fan, and resembling the leaves of *Ricinus*. This completely roves that the plant producing the true rhubarb was still nknown, which is probable enough when the difficulties are condered in the way of penetrating into the kind of Thibetian sancuary where the lamas jealously cultivate their semi-sacred plant, f which they keep to themselves the production and the profit. ike Boerhaave and Pallas, the most recent European travelers, cluding those of the Mékong Expedition, have only learned by port of the portion of Thibet, protected by inaccessible and iled-up rocks, whence caravans brought the drug to China, with e information that it grows toward the west—far from the froners of the Celestial Empire. It was only in 1867 that M. Darby as able to procure plants of the best Thibetian rhubarb; a few nds were alone saved, thanks to the skill of M. Neumann, nongst the mass received by the Société d'Acclimation in the orst possible condition. One of these was planted in the garden the Faculty of Medicine, where it has succeeded admirably;

another was cultivated by M. Giraudeau, in the valley of Montmorency. There the plant soon assumed a magnificent development. It has already produced several times large inflorescences more than six feet in height, tapering to a point, and covered with white flowers, each with a deeply-hollowed receptacle doubled in size by a green grandular disk. The leaves are of large size, attaining a length of five feet; they have a semi-orbicular limb deeply five-lobed, light-green in color, and covered with a fine pubescence. In all these characters they only approach one of the known species—*R. dentatum*, a plant entirely glabrous and referred as a variety to *R. hybridum*. The plant, therefore, hitherto undescribed, deserves the name of *R. officinale*. It might be cultivated without difficulty with us; it supported last winter a severe degree of cold without appearing to suffer the least, and it has already produced an abundant supply of rhubarb which has all the physical characters of the best Asiatic kind. These characters are the color, taste, and peculiar odor—the fine, white, and lozenge-like network of the cleaned surface, and the presence in the substance of the fragments of star-like spots described in all the authorities, and which have the structure of a dicotyledonous root. There was no reason to suppose that it would be otherwise, or that any difficulty will be experienced in propagating the plants by means of the innumerable buds which it produces. The aerial portion, conical in form, and thick as one's thigh, which furnishes the drug, and which, after the Thibetian manner, has been cleansed, split, and cut into fragments, is nothing more than an aerial stem. The supposed black bark consists of the bases of the leaves and the ocreæ; in the axil of every leaf there is a bud which is available for propagating the plant. Numerous adventitious roots springing from this axis, and having their bases prolonged into the interior of the parenchyma, produce the stellate marks characteristic of the Chinese drug. The roots of the true rhubarb contain, without doubt, in their cellular, cortical, and medullary tissue, and in that of their medullary rays, the active and coloring principle: but these organs are scarcely developed, and are represented by the slender cylindrical pieces sometimes sent to Europe; frequently they are speedily decayed through a great part of their length, whilst in other species of *Rheum*, those which furnish the so-called indigenous rhubarbs, it is the root which is especially developed and which is employed. In the true Thibetian kind it is almost entirely the stem, and it is not surprising that it should be characterized by a special structure, which will be, without doubt, a practical and ready means of recognizing and distinguishing the products of inferior quality with which the world is inundated.*

7. *Boissier*, *Flora Orientalis*, Vol. II. Genevæ et Basiliæ, 1872, pp. 1159, 8vo.—The first volume was published in 1867. In a review of it at the time in this Journal, an account of the plan,

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* Translated from the Report in the 'Revue Scientifique' of Sept., 1872, p. 279, of the Meeting of the Association Française, at Bordeaux.—W. T. T. D.

extent, and high character of this work was given. That volume contained the *Thalamifloræ;* this follows with the *Calycifloræ Polypetalæ.* The great orders are, first and foremost, the *Leguminosæ*, including 757 species of *Astragulus!* and the *Umbelliferæ* which are largely represented in the East, and by a goodly number of genera,—how many cannot so readily be stated, as the genera are not numbered. *Orobus* is kept up as a distinct genus, upon the absence of contortion of the style as well as of the tendril. *Gœbelia*, a genus proposed by Bunge, is adopted for *Sophora alopecuroides* and an allied species, which much resemble our *Sophora sericea* of the American plains. *Cerasus, Armeniaca, Persica* and *Amygdalus* are retained as genera. *Liquidambar Orientalis* is said to be very nearly related indeed to the American *L. styraciflua.* The third volume, which must mainly be occupied with *Compositæ*, is in an advanced state of preparation. A. G.

8. *Hooker's Icones Plantarum*, Third Series, vol. II, part 1, 1872, contains plates 1101 to 1125, chiefly of *Rubiaceæ* and *Compositæ*, orders of late elaborated for the forthcoming second volume of the *Genera Plantarum.* There is a new *Pentachæta* (Mexican) which we should never have recognized by the style; a second *Clappia*, in foliage, involucre, and aspect not very like the original *C. suædæfolia* of the southern borders of Texas, which is also well figured; a new *Brachyactis* from the Himalaya, with diagnoses of four other species, including the original *B. ciliata* of Ledebour, our *Aster* (*Conyzopis*) *angustus*, and it is to be hoped that the augmented and restituted genus may hold; a good figure of *Aganippea bellidiflora* DC., and two or three new genera of Compositæ from Mexico. The Rubiaceous genera have no particular interest for North American Botany. We may note, however, *apropos* to *Ceratopyxis* and its synonym "*Philalanthus spicatus* Sauvalle Pl. Cub.," that the work here cited is by Charles Wright, and the printing merely superintended by Signor Sauvalle, as in fact the title sets forth, although rather obscurely. A. G.

9. *Journal of the Linnæan Society*, No. 68, Dec., 1872.—After a short article by Dr. Masters *On the Development of the Andrœcium in Cochliostema*, the number is filled by a continuation of Mr. Baker's useful re-elaboration of the *Liliaceæ*, viz: by a *Revision of the Genera and Species of Scilleæ and Chlorogaleæ*, which this fasciculus does not finish. From the conspectus it appears that the tribe *Chlorogaleæ* consists of the singular leafless climber *Bowiea* Harvey, of South Africa, the Californian Soap-plant *Chlorogalum*, and of *Nolina* of Georgia. A. G.

10. *The Journal of Botany, British and Foreign*, December, 1872.—Mr. Trimen, one of the editors, contributes an article on *Psamma Baltica*, with a plate. It is remarked that in the points in which this grass differs from *P. arenaria*, it approaches *Calamagrostis Epigeios*, and so confirms the union of *Psamma* with the latter genus, which is already consummated almost everywhere out

of England; and a note records the fact that Host's name *Ammophila* was the earlier published, but, it is added, that this name, "having been previously used by Kirby, in 1794, for a genus of *Hymenoptera*, has been generally abandoned, in accordance with a rule which should be, though it is not, universally followed." We should have thought that the contrary expressed both the practice and the rule of the present day. An article by Baillon upon the Rhubarb plant is translated from the *Revue Scientifique* for September. This we have reproduced. A. G.

11. *Discharge of the Seeds of Witch-Hazel* (*Hamamelis*).—The common statement in the books is that the capsule or its internal bony lining bursts elastically. The clearest of them, as respects the structure of the fruit, is in Torrey & Gray, Fl. N. America, p. 596, viz: "opening at the top by loculicidal dehiscence, the valves at length two-cleft: endocarp coriaceous, separating and enclosing the seed, at length bursting elastically into two pieces. The forcible discharge of the seed is thus stated by Elliott, Sketch, 1, p. 219, "[endocarps] cells of a bony consistence, polished within, discharging the seed when mature, with a spring to some distance." In a recent communication to the Academy of Natural Sciences at Philadelphia and to the Gardeners' Chronicle, which has been extensively copied, Mr. Meehan describes the projection of the seeds, which was new to him, in an interesting and correct way, but with an unfortunate slip in describing the parts concerned. The cause of the discharge he "found to be simply in the contraction of the *bony albumen which surrounded the seed.* The seeds were oval and in a smooth bony envelope, and when the albumen had burst and expanded enough to get just beyond the middle, where the seed narrowed again, the contraction of the albumen caused the seed to slip out with force, just as we would squeeze out a smooth tapering stone between the finger and thumb."

Substitute "endocarps" or "bony internal portion of the cells," and it will be clear that the projection of the seeds is effected just as in a violet (as is mentioned under the ordinal character in Gray's Manual), only in that the whole valve folds in conduplicately with the same result. It is interesting to notice how Mr. Meehan's account has been copied over and over in scientific publications, without any attempt to render it scientifically intelligible, except in one instance, viz.: in an article in Popular Science Review, reprinted in Popular Science Monthly for January (p. 292). Here the author, Mr. Bennett, interpreting the original statement upon the assumption that the term "albumen" was correctly employed, is consistently led into the astonishing statement that "the embryos of the seeds of the American Witch-Hazel are thrown out with such force as to strike people violently in the face who pass through the woods!" A. G.

12. *Chlorodictyon, a new Genus of the Caulerpa Group;* by J. G. AGARDH, with a plate,—which represents the *Ramalina retiformis* Menzies, a Lichen of the coast of California; extract from *Ofersight Kongl. Vetenskaps-Akademiens Förhandlingar*,

1870, No. 5, Stockholm. The oversight may have been already corrected by the accomplished author; but, as it concerns a North American plant, it is as it were to call attention to it. A. G.

10. ***Braun on Marsilia and Pilularia.***—In the Proceedings (Monatsbericht) of the Royal Prussian Academy of Science of Berlin, for August, 1872, Prof. Braun gives a full account of the species of these genera, especially of those cultivated by him in the Berlin Botanic Garden, in continuation or as a supplement to his previous papers, of October, 1863, and August, 1870, and a detailed Clavis Emendata of the whole, 59 species of *Marsilia* and 5 of *Pilularia.* A. G.

11. ***Baillon: Histoire des Plantes.***—The latest part of this series of monographs which we have seen contains the *Tiliaceæ*, *Dipterocarpeæ*, *Chlænaceæ* and *Ternstrœmiaceæ*, pp. 161–264 of the fourth volume, with the usual good woodcuts. In the systematic treatment Bentham and Hooker's Genera Plantarum is closely followed. A. G.

12. *Les Mélastomacées;* par J. TRIANA, M.D. 1871. Forms Part I of the 28th volume of the *Transactions of the Linnæan Society of London*, and received the Candollean prize for the best monograph of a Genus or Family of plants. Fills 188 pages 4to, the systematic part in double columns of small type. While yet unfinished it formed the basis of the treatment of this beautiful and very difficult order in Bentham and Hooker's Genera Plantarum. It is now made complete, the species all enumerated and classified, and new ones characterized. There is a full index, and seven plates in good lithography are filled with dissections of flowers. The systematic views and criticisms appear to be thoroughly sound. A. G.

13. ALLMAN, G. J.: *A Monograph of the Gymnoblastic or Tubularian Hydroids.* Parts I, II. Ray Society, 1872.—The Ray Society has recently issued the second part of Allman's Monograph of the Tubularians. Prof. Allman, as is well known, has been occupied for many years in working up this group of animals, and the monograph he now publishes is a truly magnificent production, illustrated with beautifully engraved plates, by Wagenscheiber of Berlin, and numerous wood-cuts. But well as they are engraved, they owe their excellence to the fidelity and beauty of the drawings of Prof. Allman, who has himself drawn from life the exquisite forms which his facile pencil has given us with such wonderful accuracy. His skill as a draughtsman is well known from the plates accompanying his monograph of Freshwater Polyzoa, but the results obtained in the present monograph leave nothing to be desired, and these life-like plates will long serve as a model to all who may come hereafter to investigate the same subject. In the first part, devoted to the morphology, physiology, and general considerations bearing on the order of Hydroidea, the classification proposed by Allman in 1860, in his "Construction and limitation of genera among Hydroids," is adopted, with such modification as subsequent researches have rendered

necessary. The monograph contains an historical account of the principal results obtained by previous writers on the subject, showing the successive steps which have brought about the present systematic position of the Hydroidea.

We would notice here the mistake made by Allman, and all European writers, in assigning mainly to McCrady the credit of uniting the polypoid forms of Hydrozoa with the Gymnophthalmous Hydrozoa. The main points of McCrady's classification were taken for Professor Agassiz's lectures, though this was not sufficiently distinctly stated by McCrady in his paper on the Acalephs of Charleston harbor.

A very systematic terminology is introduced which, even when we do not agree with the views of Allman, cannot fail to introduce accuracy and prevent the ambiguity so common in our descriptions of Hydroids, as the new terms proposed can be readily remembered and are of easy application.

The chapters on the Morphology of the Tubularians give in a more extended form the views advanced by Allman in his report on the state of our knowledge of the reproductive system of Hydroidea, and which are to a certain extent, as far as the general presentation of the subject is concerned, the views which pass current among the most competent of the investigators of the subject, the principal difference consisting in adopting as zoöids, and not as individuals, the representatives of the different phases of development, the zoöids as a whole forming the individual.

The concluding portion of the part just issued is devoted to an account of the various phenomena of Hydroid life, such as digestion, secretion, circulation, nutrition, which are described as far as it is possible to limit and define these processes in those members of the animal kingdom.

We have an interesting chapter upon the geographical distribution of Hydroids, which, for the scanty materials as yet collected, is of course only preliminary. Allman succeeds in recognizing three provinces which he considers as well established in the great North Atlantic area, viz:. the Boreo Celtic, Northern Atlanto-American, and Mediterranean. Besides these, he has ascertained as sufficiently accurately determined a North Pacific, a West Indian, an Australian and a New Zealand province.

The material for these generalizations is, however, as yet too scanty, and limited to the Sertularians principally for facts upon which to base reliable conclusions. The limitation of species in their definite areas is very remarkable, though it may be due more to negative than to positive evidence. As far as the North Atlantic is concerned, which is better known than any other great realm, we are not yet in condition to distinguish its provinces accurately. The question of the difference of many of our American Hydroids from the British or Mediterranean is by no means so settled as has been taken for granted. In fact, present evidence rather tends to show a wide range in the distribution of our east coast species, north of South Carolina, many if not the majority probably being common to the two sides of the Atlantic. The general coincidence of Allman's provinces with the provinces recognized in the Atlantic Geographical Distribution of Echini cannot fail to be very interesting, as in one case the embryos are by far the most nomadic of any of the marine animals, while in the other case the most cosmopolitan species are reproduced by fixed sporosacs. The agency of man, carrying hydroids on the bottoms of vessels, is one important element in the distribution of some of the families. In treating of the bathymetrical distribution, Allman recognizes as an important element the surface zone, which, though one of the most important, has usually been entirely neglected; the hydroids attached to the fronds of floating Sargassum, with the accompanying pelagic fauna of Mollusca, Crustacea, Annelids, Acalephs, even of pelagic Fish, with the larval forms of so many Echinoderms and other animals living in deeper zones. "It is the zone of sunshine," of intensified life . . . where phosphorescent animals congregate in countless multitudes, and light up at night with their mysterious fire the dark surface of the sea. He then enumerates the characteristic forms of the Littoral, Laminarian, Coralline, deep-water and abyssal zones. In regard to the distribution in time he accepts the Hydroidean affinities of the Graptolites, and reviews carefully the whole subject as presented by Hall, Barrande, Carruthers, and Richter, discusses the affinities of several types referred to Hydroids on insufficient grounds, and adopts the views of Haeckel of the affinities of the fossil Medusæ, described by him from the lithographic slates of Solenhofen.

As regards the classification of the Hydroidea, Allman gives us in detail his views of the orders and families, extending the ordinal divisions to the Cœlenterates as a whole. It is perhaps out of place to discuss his views of the ordinal subdivisions, further than to call attention to them, and question in the present state of our knowledge the accuracy of the reduction to two orders (omitting the Madreporaria) of the Actinozoa and the reduction of the Ctenophoræ to a simple order of the same value as the Siphonophora and Lucernariæ.

Allman adopts the views of the homologies of the chambers of an Actinia and of the radiating canals of Medusæ, first proposed by Agassiz, as giving the key of the affinities of the Actinozoa and Hydrozoa, though he does not accept the extension of these homologies and their application to Echinoderms as proposed by Agassiz.

Allman then gives us an interesting chapter on the tertology and pathology of the Hydroids, and completes his masterly general exposition of the Hydroidea with the anatomy of special forms selected so as to give good examples of the more important morphological variations to be found among the Tubularians, all of which, it is needless to say, have been examined by Allman.

The second part consists of a description of the genera and species of the Gymnoblastida, in which we find a fullness of description not only of the individual Hydroid, but of all the different parts which at any time become disconnected. It is only in a few of the later publications on the subject that any attempt has been made to recognize as parts of a single individual its independent constituents, and with few exceptions the authors who have studied Acalephs found it more convenient to treat of the independent parts separately, a most unphilosophical method, which has now, we trust, received its death blow. It is sincerely to be hoped that hereafter we shall not be treated to exhaustive descriptions of disconnected Hydroids, or of Trophosomes or Gonosomes, which do not present some specially interesting features.

Like all the publications of the Ray Society, this monograph is beautifully printed, on excellent paper, the mechanical execution is excellent, and the typography, as well as the beauty of the illustrations, will make the study of this exhaustive memoir a real pleasure to all who may have occasion to consult it. A. AG.

14. *Illustrations of North American Entomology;* by TOWNSEND GLOVER. *Orthoptera.* Washington, 1872.—The author very modestly states that it is not his design to present scientific or highly finished engravings of Orthoptera, but merely figures, giving a general idea of their form, size and color, to aid the young entomologist in the identification of species. He has, however, accomplished much more than this, and produced a work which will be of permanent value to every specialist interested in the Orthoptera. The work contains thirteen plates crowded with well executed and carefully colored natural size figures of a large part of all the species known to the United States and Canada. There are no descriptions or systematic account of the species, but, as the figures have all apparently been made from carefully identified specimens, they furnish very ready means—which was before entirely wanting—for the determination of the species. In the explanation opposite each plate, the earliest name, as well as the modern one adopted, is given, with references to Mr. Scudder's principal works, and to Mr. Thomas' recent papers, where the species are described by him. The letter-press contains, besides the explanation of the plates, a few notes on the food and habits of Othopthera, a list of the animal and vegetable substances injured by them, and a good index to the plates. S. I. S.

15. *On some Elementary Principles in Animal Mechanics.*—No. IV. *On the difference between a Hand and a Foot, as shown by their Flexor Tendons;* by the Rev. SAMUEL HAUGHTON, F.R.S. —The fore feet of vertebrate animals are often used merely as organs of locomotion, like the hind feet; and in the higher mammals they are more or less "cephalized," or appropriated as hands to the use of the brain.

The proper use of a hand, when thus specialized in its action, is to grasp objects; while the proper use of a foot is to propel the animal forward by the intervention of the ground.

In the case of the hand, the flexor muscles of the fore arm act upon the finger tendons, in a direction from the muscles toward the tendons, which latter undergo friction at the wrist and other joints of the hand, the force being applied by the muscles to the tendon above the wrist, and the resistance being applied at the extremities of the tendons below the wrist by the object grasped by the hand.

From the principle of "Least Action in Nature" we are entitled to assume the strength of each portion of a tendon to be proportional to the force it is required to transmit; and since, in a proper hand, these forces are continually diminished by friction, as we proceed from the muscle to the fingers, we should expect the strength of the tendon above the wrist to be greater than the united strengths of all the finger-tendons.

Conversely, in a proper foot, the force is applied by the ground to the extremities of the tendons of the toes, and transmitted to the flexor muscles of the leg, by means of the tendons of the inner ankle, which undergo friction in passing round that and the other joints of the foot. In this case, therefore, we should expect the united strengths of the flexor tendons of the toes to exceed the strength of the flexor tendons above the heel.

In the case of the hand, friction acts against the muscles; in the case of the foot, friction aids the muscles.

I have measured the relative strengths of the deep flexor tendons of the hand above and below the wrist in several animals, and also the relative strengths of the long flexor tendons of the foot above and below the ankle in the following manner:—

I weighed certain lengths of the tendons above the wrist and ankle, and compared these weights with the weights of equal lengths of the flexor tendons of the fingers or toes, assuming that the weights of equal lengths are proportional to their cross sections, and these again proportional to the strengths of the tendons at the place of section. The difference between the weights above and below the joint represents the sum of all the frictions experienced by the tendons between the two points of section.

The following Tables contain the results of my measurements:

TABLE I. *Friction of Long Flexor Tendons of Toes.* (Cross section of toe tendons greater than cross section of muscle tendons.)

		Amount of friction. per cent.			Amount of friction. per cent.
1.	Pyrenean Mastiff	65·4	17.	Australian Dinjo	33·8
2.	African Lion	59·0	18.	Japanese Bear	31·7
3.	Common Fox	57·6	19.	Virginian Bear	25·9
4.	African Jabiru	56·8	20.	Common Llama	25 9
5.	American Rhea	52·4	21.	Hedgehog	25·0
6.	Indian Jackall	49·2	22.	African Ostrich	24·6
7.	American Jaguar	49·2	23.	Common Otter	19·8
8.	New Zealand Weka Rail	47·5	24.	Man (mean of 5)	16·2
9.	Silver Pheasant	47·4	25.	Spider-Monkey	12·3
10.	Bengal Tiger	46·0	26.	Goat	9·5
11.	Indian Leopard	45·5	27.	One-horned Rhinoceros	9·0
12.	Six-banded Armadillo	44·4	28.	Negro-Monkey	8·0
13.	Three-toed Sloth	42·5	29.	Brahmin Cow	6·8
14.	Black Swan	36·0	30.	Nemestrine Macaque	2·0
15.	Common Hare	36·0	31.	Boomer Kangaroo	0·0
16.	European Wolf	34·0			

The foregoing animals all realize the typical idea of a true foot, with a variable amount of friction at the ankle-joint; this friction disappearing altogether in the boomer kangaroo, whose method of progression realizes absolute mechanical perfection, as no force whatever is consumed by the friction of the flexor tendons at the heel.

The only animals whose feet deviated from the typical foot were three, viz: alligator, common porcupine, and phalanger. In these animals the foot has the mechanical action of a hand, or grasping organ; and the flexor tendons above the ankle exceeded those below the ankle by the following amounts:—

1.	Alligator	11·5 per cent.
2.	Common Porcupine	20·0 "
3.	Phalanger	29·2 "

In the case of the flexor tendons of the hand, I obtained the following results:—

TABLE II. *Friction of Deep Flexor Tendons of Hand.* (Cross section of muscle tendons greater than cross section of finger tendons.)

		Amount of friction. per cent.
1.	Common Porcupine	71·0
2.	Sooty Mangaby	49·2
3.	Nemestrine Macaque	40·7
4.	Capuchin Monkey	35·3
5.	Virginian Bear	35·0
6.	European Wolf	31·4
7.	Japanese Bear	30·6
8.	Negro-Monkey	27·4
9.	Spider-Monkey	26·5
10.	Bengal Tiger	22·7
11.	Common Fox	20·7
12.	Pyrenean Mastiff	7·0
13.	Goat	0·0

It will be observed that the fore foot of the goat, regarded simply as an organ of locomotion, attains a perfection comparable with that of the hind foot of the kangaroo, no force being lost by friction at the wrist-joint.

The only animal in which I found a departure from the typical hand was the llama, in which the flexor tendons of the fingers exceed the flexor tendon above the wrist by 14·4 per cent.

The bearing of the foregoing results on the habits of locomotion of the several animals will suggest themselves at once to naturalists who have carefully studied those habits. I shall merely add that the subject admits of being carried into the details of the separate or combined actions of the several fingers and toes, and that the habits of various kinds of monkeys in the use of certain combinations of fingers or toes may be explained satisfactorily by the minute study of the arrangement and several strengths of the various flexor tendons distributed to the fingers or toes.

III. ASTRONOMY.

1. *Meteors of Nov. 27th,* 1872, *in Europe.*—In the last number of this Journal were given accounts of meteors seen on the evenings of Nov. 24–27th ult. in this country. The steadily decreasing numbers on the evening of Nov. 27th, naturally suggested the thought that we were witnessing but the close of a

far more brilliant display, the full benefit of which had been enjoyed in Europe. The shower was in fact the most considerable in respect to the numbers of shooting stars which has occurred since 1833. One account from Padre Denza of Moncalieri has been given in this number of the Journal. From the *London Mail* of Nov. 29th, we quote from the account of Mr. E. J. Lowe of Highfield House, near Nottingham, England, who is an experienced observer.—"One of the most remarkable showers of falling stars that has ever been seen in England was witnesssd here last evening, continuing from 5.20 P. M. (when it was observed) till 10.30 P. M. (when the sky became overcast, and continued so till 4 A. M. of to-day). Taking into consideration the great number of meteors that were seen at 5.40 P. M. (being equal to those seen at 8 P. M.), it seems probable that this shower may have commenced some two hours earlier. The meteors were counted on a quarter of the visible sky, and as each of the four quarters of the heavens were similarly repeatedly counted, and the average number seen being nearly equal, it follows that four times the number were falling that could be observed by any one person. The following brief extract will give the numbers actually counted in the space of one minute of time:—

Time. h. m.	Meteors per minute.	Time. h. m.	Meteors per minute.
5 50	83	8 4	71
6 1	61	8 20	59
6 11	69	8 35	39
6 15	91	8 50	20
6 20	104	9 5	18
6 30	111	9 15	31
6 50	101	9 30	20
7 10	61	9 50	16
7 29	84	10 10	12
7 45	60	10 30	6
7 55	120		

Average number per minute in a fourth of the heavens:—

h. m. h. m.	Number.	h. m. h. m.	Number.
5 50 to 6 15	74	8 20 to 8 35	46
6 15 to 6 20	97	8 35 to 9 15	27
6 20 to 6 50	105	9 15 to 9 30	25
6 50 to 7 10	81	9 30 to 9 50	18
7 10 to 7 55	81	9 50 to 10 10	14
7 55 to 8 4	95	10 10 to 10 30	9
8 4 to 8 20	65		

The estimated number of meteors in these 13 periods gives the following results:—

	Meteors in one-fourth of the sky.	Probable No. in the whole sky.		Meteors in one-fourth of the sky.	Probable No. in the whole sky.
1st period	1,850	7,400	9th period	810	3,240
2d "	840	1,920	10th "	375	1,500
3d "	3,150	12,600	11th "	360	1,440
4th "	810	3,240	12th "	280	1,120
5th "	3,645	14,580	13th "	180	720
6th "	950	3,800			
7th "	1,040	4,160	Total,	14,665	58,660
8th "	735	2,940			

Showing the immense number of 58,660 meteors as the probable number that actually fell between 5.50 P. M. and 10.30 P. M. (i. e., in 4h. 40m.), and from the difficulty in counting the smaller meteors I am inclined to place these figures below the actual amount visible during this new star-shower display. The radiant point was very carefully watched, and considered to be a circular area of about 1 deg. in diameter (or rather less), the center of which was situated in A. R., 2 h. 45 m.; declination, N., 46 deg. 15 min. With very few exceptions, all the meteors could be traced to this area, and not less than 20 were seen to blaze and die away on the spot without moving among the stars. At 8 h. 52 m. a red meteor was noticed quite close to γ Andromeda, which attained a *maximum* brightness equal to that star, then faded away without moving. No other motionless meteors were seen except those about the radiant point. Near the radiant point the meteors were the smallest, and had the shortest paths among the stars, increasing in size, brightness, and length of path as their distance from this point increased. The principal feature in the early part of the display was the extreme smallness of the great portion of the meteors, not one in ten being equal to a star of the third magnitude, and many were as minute as the smallest visible stars, and might be aptly called meteor-dust. As the period advanced there was a marked (though gradual) increase in size and brightness, especially after 7 P. M., and in those meteors considerably removed from the radiant point. There was also a remarkable similarity between the meteors; all had tails, and, indeed, with the exception of the larger ones, and those most minute, they might be said to be all tail. None but the largest falling stars were observed to vary in color from that of the ordinary color of the fixed stars, and a large proportion assumed the appearance presented by a descending rocket-stick. The meteors differed considerably as regards apparent speed from those of the ordinary November epoch, being not nearly so rapid, nor, as a rule, did they leave so continuous a streak in the sky; in fact, but few exhibited continuous streaks, and these were all those meteors that were colored mostly red. Four or five times during the display (and more frequently after the sky became cloudy, even up to 5 A. M.) there were noises in N.W. and W.N.W., which closely resembled the reports of very distant gun shots; but whether connected with the phenomenon I am unable to say, though the same kind of noise was heard in the last great star-shower; and once, at 7h. 31m., this report followed by, perhaps, an interval of a minute a discordant red meteor, which moved rapidly from Polaris to near Vega. This meteor shot very rapidly, and left a streak in the sky visible for a second of time throughout the whole interval between these two stars. Its size exceeded a first magnitude star. At 7h. 55m. another red meteor, equal to a second magnitude star, moved from near the radiant point across Cassiopeia, leaving a streak that was visible for two minutes. Streaks lingered in several other instances for a considerable time, though this was an exception to the rule. The showers of falling stars were in impulses of short periods, but they were not regular. Mostly the impulses were in nearly half-minute periods, with greater impulses every five or six minutes. Between 6h. 30m. and 7h. 30m. these impulses were mostly in 20 second periods, each time lasting about 8 seconds. There was an evident tendency in the meteors to follow each other, sometimes even three and four pursuing the same path rapidly, one after another."

The observations of Dr. J. F. Julius Schmidt of Athens, began an hour earlier than those of Mr. Lowe, and owing to a very clear sky they were continued much later, even overlapping those made in America. Dr. Schmidt has been an indefatigable observer of shooting stars for thirty years, and is accustomed to apply an important correction to the observations of his assistants, which is by others disregarded. Of two observers one will usually see more shooting stars than another, because of better eyesight. For his assistants, Dr. Schmidt has by trial obtained factors expressing this difference and applied them to his observed numbers. He thus obtains the following numbers, exhibiting the intensity of the shower. The symbol z expresses the number of shooting stars seen per hour by one observer at the time indicated.

For $6^{h}\cdot0$	$z = 375$	For $9^{h}\cdot0$	$z = 1760$	For $12^{h}\cdot0$	$z = 590$
" 6 ·5	$z = 650$	" 9 ·5	$z = 1610$	" 12 ·5	$z = 440$
" 7 ·0	$z = 980$	" 10 ·0	$z = 1425$	" 13 ·0	$z = 300$
" 7 ·5	$z = 1300$	" 10 ·5	$z = 1200$	" 13 ·5	$z = 200$
" 8 ·0	$z = 1620$	" 11 ·0	$z = 1020$	" 14 ·0	$z = 125$
" 8 ·5	$z = 1770$	" 11 ·5	$z = 810$	" 14 ·5	$z = 64$

The value of z is to be understood thus: between seven and eight o'clock one observer could see 1300 meteors, and so for the other hours. This table gives for the whole number visible at Athens during the 9 hours as about 8195 for one person. From this Dr. Schmidt estimates at least 25,000 in all for that period, or 30,000 for the whole night. He adds:

" Whilst the shower of Nov. 13th, 1866, was remarkable for the brilliancy of the meteors, for so many hundred of *bolides*, for the slenderness and brightness of the trains, for the preponderance of green colors, and for the rapidity of the apparent motions, that of Nov. 27th, 1872, was wholly different. By far the largest number of the shooting stars were faint,—5th to 6th magnitude,—the trains were numerous but broad and smoke-like, so that many of the larger meteors were very like comets with long tails. The green colors were seen hardly once, but the colors were between white and reddish-yellow. Many thousands were orange or reddish-yellow. Especially remarkable seemed to me the slow motion (without exception), the often *undecided* motion, and the fact established by hours of observation, that nearly all the meteors which were not more than 30° or 40° distant from the radiant had curved or otherwise anomalous paths. From about 1,000 such paths, I had continually the impression that besides the ordinary motion, which often lasted from 1^{s} to $2{\cdot}5^{s}$, there was in addition a marked drift sideways, which I had in previous years (though rarely) perceived, and which I had regarded as an effect of the earth's rotation.

"Of brilliant fireballs, those on my scale marked as I and II, I saw none. Only 2 or 3 had the brightness of Jupiter, 5 or 6 that of Sirius, perhaps 200–300 of first magnitude; all the rest were faint, and rarely was a trail wanting. Even in the first hour we saw not only frequently 5 or 6 meteors in a second, but simultaneously in different parts of the sky groups of 3–7 running in parallel paths. After $8\frac{3}{4}^h$ we saw small groups of 10–20, together, and one after the other, not only near the radiant (where they streamed out in troops on all sides in flowing pulsating motion, often only as short lines of light), but also at distances of 50° or 60° from the radiant. * * * Twice I saw a meteor of the second magnitude, with which traveled 7 or 8 small shooting stars of the 6–5th magnitude close to it and as it were in its broad trail. None of them preceded the principal meteor; all followed at a small distance. It was to the naked eye just such an appearance as was presented in the telescope by the meteor seen at Athens Oct. 18th, 1863." (*Astr. Nach.*, No. 1915.)

The European journals contain a large number of accounts of the shower as seen in England, Germany, France, Italy, and even farther east, in Egypt and Constantinople.

In the last number of the Journal it was urged that the actual orbits of the individuals of each meteor-group intersected at a considerable angle, and that this would imply that the disintegrating cause was one acting upon comets at the perihelion. The observation of Dr. Schmidt, that so many paths were curved, is important as indicating that the glancing of the meteors upon entering the air is so common as *perhaps* to be the principal cause of the magnitude and shape of the radiant area. We saw curved paths, but by no means so many in proportion as Dr. Schmidt.

It is possible that the observations of some of the European observers are of such a nature as to give evidence upon this important point.

If the meteors are moving in parallel paths before entering the air, and there is a radiant area of definite shape, other than circular, it should be, at any specified hour, symmetrical with respect to the horizon.

It would seem probable that the general amount of the dispersion of the radiation due to glancing in the air should increase or decrease with the velocity. Thus the velocity of the August meteors is probably intermediate between those of Nov. 14th and Nov. 27th. We should naturally expect the dispersion due to this cause in the August group, therefore, to be intermediate between that in the two November groups. This is not the case. Perhaps, however, the particles are more angular in one group than in another. Other possible causes can easily be suggested.

We are very glad to add (from *Nature*) a letter from Mr. Pogson to the Astronomer Royal, dated Dec. 5, 1872, showing that one portion of Biela's comet was visible on the 2d and 3d of December:

"Biela's comet is my subject this time. A startling telegram from Prof. Klinkerfues on the night of Nov. 30th ran thus: 'Biela touched Earth on 27th: search near Theta Centauri.'

"I was on the look-out from comet-rise (16^{h}) to sunrise the next two mornings, but clouds and rain disappointed me. On the third attempt, however, I had better luck. Just about 17¼^{h} mean time, a brief blue space enabled me to find *Biela*, and though I could only get four comparisons with an anonymous star, it had moved forward 2^{s}·5 in four minutes, and that *settled* its being the right object. I recorded it as "Circular; bright, with a decided nucleus, but no tail, and about 45″ in diameter." This was in strong twilight. Next morning, Dec. 3, I got a much better observation of it; seven comparisons with another anonymous star; two with one of our current Madras Catalogue Stars, and two with 7734 Taylor. This time my notes were "Circular; diameter 75″; bright nucleus; a faint but distinct tail, 8′ in length and spreading, a position angle from nucleus about 280°." I had no time to spare to look for the other comet, and the next morning the clouds and rain had returned.

"If I get another view before posting this, I may be able to add a hasty postscript. The positions, the first rough, the second pretty fair, from the two known stars, are—

	Madras M.T.			R.A.			(Apparent) P.D.		
	h	m	s	h	m	s	°	′	″
Dec. 2	17	33	21	14	7	27	124	46	
3	17	25	17	14	22	2·9	125	4	28

H. A. N.

2. *On a new Meteorite found in Indiana;* by Professor Cox.—At a meeting of the Indianapolis Academy of Sciences, Nov. 20, 1872, Professor E. T. Cox presented a paper on a hitherto undescribed meteorite, which was found in the year 1870, in digging a well on Mr. Freeman's farm, seven miles southeast of Kokomo, in Howard county, Indiana, by Dr. Saville, who now lives in Sioux City, Iowa; and we are indebted to him for its preservation. It was presented to Professor John Collett by the Doctor last August, when he visited Sioux City, and it has been loaned to Professor Cox for examination and description.

The depth at which it was found in the well could not be satisfactorily learned; but from being imbedded in plastic clay, which lies beneath a bed of peat, the probability is that, in falling, it met with no very great resistance until it reached this clay. It is a flattened, irregularly shaped mass, rounded on one side and concave on the other; the surface is darkened and covered with slight indentations. The dimensions are: greatest length, five inches; average width, three and a half inches; average thickness, one inch and seven-tenths; its weight is four pounds one and a half ounces, avordupois. A small piece has been cut from one edge, and it is said that the smith broke two chisels in the operation. The fracture is granular, like fine steel, and the cut surface has a silvery appearance; it is malleable and somewhat harder than common bar iron, and, like the latter, it may be wrought into all manner of shapes. This meteorite only came into the Professor's hands a few days ago, and, owing to press of other work, he is able at this time to give nothing more than the result of a partial chemical analysis. It is destitute of stony matter, and the principal element is iron; next comes nickel; then, in small quantities, cobalt, tin, carbon, phosphorus and probably a trace of sulphur. Submitted to the action of acids, the Widmanstattian figures are brought out in great perfection. At what time this meteorite fell is not known, but it is hoped that by calling the attention of the citizens of Howard county to the subject, we may yet receive information regarding its history which will still further add to its scientific value.

IV. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *Note by* Prof. JOSEPH LECONTE. (From a letter to the Editors, dated Oakland, California, Jan. 13, 1873.)—In the Nov. number of your Journal appeared simultaneously my paper on "The formation of the great features of the earth's surface" and an abstract of Mr. Mallet's paper on "The source of volcanic energy." Mr. Mallet's paper was, I believe, read before the Royal Society, June 20, 1872, although I did not see any account of it until October. I have not yet seen Mr. Mallet's full paper, but from the abstract I observe that there are several points common to the two papers. I wish, therefore, to make a brief statement of certain circumstances connected with the publication of my paper.

The views contained in my paper I have taught in my geological class for three years past. In Jan., 1872, I elaborated them in the form presented in your Journal, and gave them as a lecture to my 1st class in geology; from the notes of this lecture my paper was afterward written. I finished it, as you will observe by the date, May 15. It was in your hands early in June, and I expected it to appear in July. Your July number was already full before you received my article; correspondence between this and New Haven is tedious, requiring 3 weeks to receive a reply to a letter. I was absent from Oakland in the high Sierras during a part of July, the whole of August and a part of September, so that I could not correct proofs as I desired; the publication was therefore unavoidably put off until my return. Thus it has happened that a paper which was completely elaborated and given as a lecture in January, which was written out and finished and dated in May, and in the publisher's hands in June, did not appear until November.

I mention these facts not with any view to reclamation, but simply to show the complete independence of my views on this subject.

In justice to Mr. Vose I ought also to draw attention to the fact that Mallet's very excellent idea that *heat is produced by pressure* is brought out distinctly in his (Vose's) little volume on Orographic Geology, published in 1866. Mr. Vose, however, did not extend the idea to vulcanism, but only to metamorphism.

2. *Syracuse University.*—Prof. A. WINCHELL, long connected with the departments of Geology and Natural History in Michigan University, and till recently in charge of the Geological Survey of the State, has just entered on the duties of Chancellor of the new institution at Syracuse called the Syracuse University, chartered by the State in 1870. The University has already received, through the liberality of its friends, property amounting in value to about $650,000.

3. *The Earth a Great Magnet;* by ALFRED MARSHALL MAYER, Ph.D., Professor of Physics in the Stevens Institute of Technology. New Haven, C. C. Chatfield; London, Trübner and Co.—Instead of writing a notice ourselves of Prof. Mayer's work, we present the following from the Philosophical Magazine of January, p. 65:

This is the report of a lecture delivered before the Yale Scientific Club on February 14, 1872, in which the lecturer proposed to present to his audience "*one prominent truth* in simple and striking experiments." The truth which is kept steadily before the mind throughout the lecture is, "that the earth is a great magnet;" and this truth is developed, step by step, by experiments of the most conclusive kind, each having been rendered distinctly visible to the audience by means of the *vertical* lantern, so that the processes of magnetizing and demagnetizing, with all the interesting motions of the needles, were seen projected on a luminous screen of eighteen feet diameter.

The lecture itself is a masterly production, and exhibits the result of much close reading as well as experimental research. Quotations are given from earlier writers on magnetism, illustrative of the sound knowledge which they possessed; and as each experiment illustrative of the lecture is described as well as the apparatus employed in manipulation, the reader is conducted from a consideration of the most ordinary magnetic phenomena presented by bar and electro magnets, to that of the same phenomena evolved from terrestrial magnetism. A paragraph selected from the closing portion of the lecture will fully substantiate this statement.

"Now we have finished our experiments; and what have they shown? I have temporarily magnetized a bar of soft iron, by pointing it toward a pole of our large magnet. I did the same with the bar and the earth. I permanently magnetized an iron bar, by directing its length toward the pole of the magnet, and vibrating it with a blow of a hammer. I did the same with a bar, struck when pointed toward the earth's magnetic pole. I have shown you the action of a small magnetic disk on iron filings placed above and around it. You saw that the earth produced the same action on the beams of the aurora. I showed you the action of this disk on a freely suspended magnetic needle, and pointed out to you the earth's similar action on a dipping-needle carried over its surface. I have evolved a current of electricity from a magnet, by cutting with a closed conductor across those lines in which a magnetic needle freely suspended places its length. I did the same with the earth by cutting across those lines which are marked out by the pointing of the dipping-needle. Therefore, what am I authorized to infer? When the effects are the same, the causes must be the same; for according to all the principles of philosophy, and conformably to that universal experience which we call common sense, like causes produce like effects."

To those who are desirous of possessing in a compressed form the leading facts of terrestrial magnetism, we strongly recommend a perusal of the lecture.

4. *Illustrated Catalogue of the Museum of Comparative Zoölogy.* No. VII. *Revision of the Echini;* by ALEXANDER AGASSIZ. Parts I and II, with 49 plates. Cambridge, Mass., December, 1872.—This is an admirable work, and by far the most important one yet published upon the Echini. Part I contains a very complete bibliography; a chapter on nomenclature; a chronological list of all the genera and species hitherto named, each name, when it first appears, being printed in black-faced type; an alphabetical list of all the genera and species, with their full synonymy, and a list of the authenticated localities of each; a synonymic index to the preceding list, in which all names, whether synonyms or not, are alphabetically arranged; and an account of the geographical distribution, including a list of all the known species with their geographical range, and special lists of the littoral species found in the various districts and faunæ. The geographical distribution is illustrated by seven plates, printed in colors, some of them showing the division of the oceans into littoral faunæ and great realms, and others illustrating the distribution of the principal genera. These have been very carefully and skillfully executed, and add much to the value of the work. In all the lists the system of printing each original name in heavy-faced type, to distinguish them from names adopted by a subsequent writer, has been adhered to, and will be of great convenience to those who have occasion to study the synonymy.

In the investigation of the synonymy, Mr. Agassiz has had unusual facilities for the examination of original and authentic specimens, for besides the unrivaled collections of the Museum of Comparative Zoölogy, he has had excellent opportunities to study all the principal collections of Europe, and has thus examined the original specimens, even of many of the earliest writers on Echini. In this, the most laborious part of his work, he has evidently spared no pains to make the synonymy as accurate and complete as possible.

The various perplexing questions that invariably arise in unraveling the early synonymy, especially of genera, have been treated very judiciously, and though others may not agree with the author as to the names to be adopted in special cases, he has presented all the data that is needed to decide the questions, let the principles of nomenclature that we adopt be what they may.

The point that will be most opposed is, perhaps, the restoration f certain ante-Linnæan names, chiefly those of Klein, whose oriinal specimens Mr. Agassiz has been able to study.[*]

Part II contains detailed descriptions of all the species known) inhabit the eastern coast of the United States, including all the eep-sea species dedged by Mr. Pourtales during the Coast Surey explorations.

In this part of the work are many interesting details concerning ıe development of the young. The descriptive portion is fol›wed by tables showing the bathymetrical[†] and geographical disribution of the species, and by a systematic list giving their lassification.

The forty-two plates illustrating this part of the work are truly dmirable, and aside from their great scientific value are worthy f special attention, because they show the successful application f two new and valuable processes of photo-engraving to the lustration of zoölogical subjects. Many of these plates have all he minute details and the precise shading of the original photoraphs, from which, indeed, they can scarcely be distinguished. 'hese were done by the "Woodburytype Process," by the American 'hoto-Relief Co., Philadelphia, from negatives by Mr. Sonrel of loston. Others, which are quite different in character, but also xcellent in quality, were done by the "Albertype Process," by he Photo-Plate Printing Co., New York. These are partly from hotographs of the objects and partly copied from drawings. 'here are also some excellent lithographs.

We are gratified to learn that the remaining parts of this work, ontaining descriptions of the foreign species and the anatomy of he Echini, are already well advanced, though their publication ill be somewhat delayed, owing to the loss of several plates, ith the original drawings, by the great fire in Boston. The illusrations will be similar in character to those of the volume just ssued.

A. E. V.

5. *Wagner's Chemical Technology;* translated and edited 'om the Eighth German edition, with extensive additions; by VILLIAM CROOKES, F.R.S., with 336 Illustrations. 745 pp. 8vo. 872. New York: D. Appleton & Company.—An English renering of Wagner's well known *Handbuch der chemischen Techno›gie* is an important addition to our chemical literature. The nnual reports on the same subject by Prof. Wagner are familiar) all students in technical chemistry, as the equivalent of the ahresbericht for general chemistry originated by Berzelius and ontinued by Liebig and Kopp. The first edition of the present

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[*] This question has been quite fully and ably discussed by Dr. Lütken in a rmer number of this Journal, Vol. III, page 382.

[†] In this connection it may not be out of place to mention that three of the ecies have had their range in depth greatly extended during the past season. ı the table on page 368, Mr. Agassiz gives 80 fathoms as the greatest depth for *trongylocentrotus Dröbachiensis* and *Schizaster fragilis*, and 40 fathoms as the mit of *Echinarachinus parma.* These were all dredged by Messrs. Smith and arger in 430 fathoms, off St. George's Bank, on the Coast Curvey steamer, ache, as stated in the last number of this Journal. This fact was communicated › Mr. Agassiz, by the writer, as soon as possible, but too late to be of use to him, r the text of the work was printed last August.

work appeared in September, 1850, and the seven others have followed in rapid succession. The subjects discussed are under eight divisions: I. Chemical Metallurgy, alloys and preparations made and obtained from metals: II. Crude materials and products of chemical industry: III. Technology of glass, ceramic ware, gypsum, lime and mortar: IV. Vegetable fibers and their technical application: V. Animal substance and their industrial application: VI. Dyeing and calico printing: VII. The materials and apparatus for producing artificial light: VIII. Fuel and heating apparatus. Although technology is not a science of itself, it draws its best results from the teachings of many sciences, and the thorough familiarity of Dr. Wagner with the latest results of chemistry in the preparation of his annual reports, is reflected in the "Handbook," which for fullness and exact statement on nearly all topics presented leaves little to be desired with reference to results as late as the year 1870. The style of the work is necessarily very condensed, where so vast a field is covered in 750 pages; and references to original memoirs are omitted, as all such are supplied in great fullness in the author's annual reports. The chemical formulæ are all molecular. A French and a Dutch translation of this edition of the Handbook have also appeared. The work is indispensable to all who are engaged in technical chemistry. B. S.

5. *Alizarine, natural and artificial;* by FRED. VERSMANN, Ph.D., member of the Chemical Society, London. pp. 34. Rumpff & Lutz, 42 Beaver st., N. Y., 1872.—In this paper Dr. Versmann gives, for the benefit of the non-chemical reader, a sketch of the history of Alizarine and Anthracine, with an account of the present state of the art of producing artificial alizarine from the oxidation of anthracine derived from coal-tar pitch. This important step in technical art, which threatens to put an end to the growth of madder by substituting a better and cheaper source of its peculiar coloring principle, is largely due to the sagacity of Dr. Versmann who has perfected a process for the economical production of anthracine from coal-tar. His paper is an instructive review of the chemistry of the whole subject to which his own researches have added important contributions. B. S.

6. *Transactions of the Wisconsin Academy of Sciences, Arts and Letters,* 1870–1872. 8vo, pp. 200. Madison, Wisconsin, 1872. Published by order of the Legislature.—Besides papers in the department of the Social and Politicial Sciences, this volume contains several articles in Natural Science and Geology, by, severally, Dr. P. R. Hoy, I. A. Lapham, J. G. Knapp, Prof. J. H. Eaton, Prof. R. Irving, Prof. T. C. Chamberlin, and Rev. A. O. Wright. There is also a mathematical paper on Potentials and their applications in Physical Science by Prof. John E. Davies.

7. *Das Elbthalgebirge in Sachsen;* von Dr. HANNS BRUNO GEINITZ. Six parts of this excellent work (noticed in vol. ii, 305, and iii, 306 and 400) have now been published, the first five making the first volume, and the next commencing volume two.

APPENDIX.

On a New Sub-class of Fossil Birds (*Odontornithes*);

by O. C. MARSH.

THE remarkable extinct birds with biconcave vertebræ (*Ichthyornidae*), recently described by the writer from the upper Cretaceous shale of Kansas,* prove on further investigation to possess some additional characters, which separate them still more widely from all known recent and fossil forms. The type species of this group, *Ichthyornis dispar* Marsh, has well developed *teeth in both jaws.* These teeth were quite numerous, and implanted in distinct sockets. They are small, compressed and pointed, and all of those preserved are similar. Those in the lower jaws number about twenty in each ramus, and are all more or less inclined backward. The series extends over the entire upper margin of the dentary bone, the front tooth being very near the extremity. The maxillary teeth appear to have been equally numerous, and essentially the same as those in the mandible.

The skull is of moderate size, and the eyes were placed well forward. The lower jaws are long and slender, and the rami were not closely united at the symphysis. They are abruptly truncated just behind the articulation for the quadrate. This extremity, and especially its articulation, is very similar to that in some recent aquatic birds. The jaws were apparently not encased in a horny sheath.

The scapular arch, and the bones of the wings and legs, all conform closely to the true ornithic type. The sternum has a prominent keel, and elongated grooves for the expanded coracoids. The wings were large in proportion to the legs, and the humerus had an extended radial crest. The metacarpals are united, as in ordinary birds. The bones of the posterior extremities resemble those in swimming birds. The vertebræ are all biconcave, the concavities at each end of the centra being distinct, and nearly alike. Whether the tail was elongated, cannot at present be determined, but the last vertebra of the sacrum was unusually large.

This bird was fully adult, and about as large as a pigeon. With the exception of the skull, the bones do not appear to have been pneumatic, although most of them are hollow. The species was carnivorous, and probably aquatic.

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* This Journal, vol. iv, p. 344, Oct. 1872, and vol. v, p. 74. Jan., 1873.

When the remains of this species were first described, the portions of lower jaws found with them were regarded by the writer as reptilian;* the possibility of their forming part of the same skeleton, although considered at the time, was not deemed sufficiently strong to be placed on record. On subsequently removing the surrounding shale, the skull and additional portions of both jaws were brought to light, so that there cannot now be a reasonable doubt that all are parts of the same bird.

The possession of teeth and biconcave vertebræ, although the rest of the skeleton is entirely avian in type, obviously implies that these remains cannot be placed in the present groups of birds, and hence a new sub-class, *Odontornithes* (or *Aves dentatæ*), is proposed for them. The order may be called *Ichthyornithes*.

The species lately described by the writer as *Ichthyornis celer* also has biconcave vertebræ, and probably teeth. It proves to be generically distinct from the type species of this group, and hence may be named *Apatornis celer* Marsh. It was about the same size as *Ichthyornis dispar*, but of more slender proportions. The geological horizon of both species is essentially the same. The only remains of them at present known are in the museum of Yale College.

The fortunate discovery of these interesting fossils is an important gain to paleontology, and does much to break down the old distinctions between Birds and Reptiles, which the Archæopteryx has so materially diminished. It is quite probable that that bird, likewise, had teeth and biconcave vertebræ, with its free metacarpals and elongated tail.

YALE COLLEGE, New Haven, Jan. 20th, 1873.

The Eruption of Vesuvius in 1872; by Professor LUIGI PALMIERI, of the University of Naples, Director of the Vesuvian Observatory. With Notes, and an Introductory Sketch of the present state of knowledge of Terrestrial Vulcanicity, the Cosmical Nature and Relations of Volcanoes and Earthquakes; by ROBERT MALLET, Mem. Inst. C.E., F.R.S., F.G.S., M.R.I.A., etc. With Illustrations. 148 pp. 8vo. London: Asher & Co. 1873.

On page 409 of last volume, an abstract is given of a memoir by Mr. Mallet, on the origin of volcanic energy and heat, now in course of publication by the Royal Society. The work, whose title is here given contains, besides a translation of the very valuable paper by Palmieri, a much fuller statement (covering 78 pp., 8vo.) of Mr. Mallet's important views, which more than double the scientific value of the volume.

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* This Journal, vol. iv, p. 406, Nov., 1872.

ERRATA.—Vol. iv, page 206, for *Telmalestes*, read *Telmatolestes*; page 219, for *Taxymys*, read *Tachymys*; page 323, for *Tinoceridæ*, read *Tinoceratidæ*; page 344, for *Dinocerea*, read *Dinocerata*.

THE

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[THIRD SERIES.]

ART. XIX.—*Observations on the duration and multiple character of Flashes of Lightning*; by OGDEN N. ROOD, Professor of Physics in Columbia College.

ARAGO has classified the different forms of lightning under three heads: 1st, linear zig-zag flashes; 2d, flashes appearing as a broadly diffused light (sheet lightening, heat lightening), and lastly, the rarely occurring discharges which are seen as slowly moving balls of fire.* That the first form is due to the production in the atmosphere of a gigantic electric spark, has since Franklin's time been disputed by no one, and as far as I can ascertain, the majority of physicists and meteorologists *suppose* that flashes of the second form are due to the same cause, their light being seen either by transmission through, or reflection from, the clouds.

Before detailing my own observations on this matter, I wish briefly to mention the results thus far obtained by others. Among the most important of them is an experiment by Dove, who, in 1835, during a thunder storm, was able, by the help of a revolving disc with colored sectors, to satisfy himself that single flashes of lightning often consisted of a number of apparently instantaneous discharges.† Arago suggested for the determination of the duration of the flash the use of a rotating circular disc, painted with a hundred black and white sectors. He supposed that from the appearance presented by the disc when illuminated by the flash, the observer would be able to ascertain its duration.[*] Wheatstone, with a revolving disc of this kind, found the duration of the flash too short for measurement, and set it as less than $\frac{1}{1000}$ of a second. Casselmann, in 1847, noticed that single flashes of lightning sometimes moved the index of the telegraphic apparatus forward over two, four, or even six letters, and argued thence the multiple character of the discharge, which indeed had previously been observed by Dove.[†] Faraday, in 1857, noticed that the duration of some flashes of lightning *seemed* to him fully as great as a second, if not more, and attempted to explain this by a phosphorescence of the portion of the cloud traversed by the flash.[‡] C. Decharme at Angers, in 1868, while a distant storm was raging, saw the heavens from time to time illuminated by a light which *seemed* to last from a half to an entire second.[§] In these last two observations no apparatus was used for actual measurement. To the above, I may add a rough measurement of the duration of the lightning flash, made by myself in 1870, with an apparatus differing from that employed by other observers, the interval of time obtained being often $\frac{1}{500}$ of a second, though sometimes it was considerably smaller.[‖] Finally, after finishing the observations and measurements detailed in the present paper, I saw in "Nature," July 25th, 1872, an account of some experiments which were made by B. W. Smith, with the aid of a "color-top," or after the method of Dove. Mr. Smith notices the fact, that the duration of the flashes sometimes seem to be prolonged by an action, which he, like Faraday, attributes to a phosphorescence of the cloud in which the discharge takes place. These observations appear at first sight to be more or less contradictory, but with the aid of the more complete set of results recently obtained by myself it will be easy to bring them into harmony.

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* See Dr. Ernst E. Schmids' Lehrbuch der Meteorologie; Leipsic, 1860, p. 781.

† Pogg. Ann., 1835, p. 371.

New Observations.

(1.) Immediately after my own experiment, I arranged a small train of toothed wheels driven by a spring, so that it should be capable of rotating a circular paste-board disc which was provided with four *open* sectors of 3° each. This apparatus was constantly near me ready for use, but an entire year elapsed before an opportunity occurred. I noticed then upon one occasion about midnight, that my room was from time to time illuminated by lightning-flashes, whose duration seemed as great as an entire second, and upon making an examination with the rotation apparatus, it was found that each flash consisted of a considerable number of isolated and apparently instantaneous electric discharges, the interval between the components being so small, that to the naked eye they constituted a continuous act. The lightning was of the second form, and judging from the accompanying thunder was not very distant, but it soon ceased, so that I was not able to gain any further information.

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[*] Becquerel, Traité de l'Electricite et du Magnetisme, vol. vi, pp. 128, 129, Paris, 1840.

[†] Fortschritte der Physik, for 1847, p. 668.

[‡] Phil. Mag., IV, xiii, p. 506.

[§] Fortschritte der Physik, xxiv, p. 644.

[‖] This Journ., III, i, Jan., 1871.

(2.) The next observations were made late one evening in last June. The storm was at a greater distance, and as before, the clouds were illuminated by unseen flashes. Each flash again consisted of a considerable number of apparently instantaneous discharges, perhaps about ten, but the total duration of the compound flash was so great, that, with the lowest practicable rates of rotation, I was entirely unable to form even a rough estimate of the interval of time involved. The storm allowed me no time for the study of the components of the flashes.

(3.) During the following month, at about the same hour in the night, I again observed near the horizon clouds illuminated by a more distant storm. The multiple flashes were once more noticed, and when the disc revolved at rates of five to ten revolutions per second, the light from the four sectors was quite often drawn out into a circular streak, which extended around its entire circumference, showing a steady and continuous duration of some of the components for at least $\frac{1}{20}$ to $\frac{1}{40}$ of a second. These streaks were observed quite often and with perfect distinctness, though the figures just given are to be considered only rude approximations. Mingled with these, if not actually constituting a portion of them, were apparently instantaneous flashes. The total duration of the act was so great as again to defeat attempts at measurement.

(4.) A thunder-storm occurring at 2.30 A. M. on July 30th gave a better opportunity. The observations, like the preceding, were made at Peacedale, R. I., and the majority of the electrical flashes were so distant that the accompanying thunder was heard only occasionally, and as before the flashes were not seen directly but by transmission. In spite of this, however, many of them were very vivid, and much rain fell during the time occupied by the observations. Previously I had arranged the apparatus for the measurement of greater intervals of time, by attaching to one of the axes a disc with a single open sector, which could be rotated at quite low rates, the terminal axis being provided with a fan. When this disc was revolving at the rate of 2·04 turns per second, the open sector was several times observed to make at least two complete revolutions during a single flash, which gave a total duration for the act in those cases of about one second, and I may add that after the completion of the experiments described below, durations of the total act were observed, even with the naked eye, which must have been as great as a second, and the light illuminating the room was noticed during the continuance to flicker twice or thrice. The total duration was, however, quite variable, and often amounted only to a small fraction of a second.

With a disc having only a single narrow, open sector, and making from 12 to 15 revolutions per second, the multiple character of the flashes was then studied. The flash consisted of a considerable number of isolated electrical discharges, which sometimes were executed with so much regularity as apparently to cause the disc to rotate backward, with a slow motion through 30° or 40°, the cause being analogous to that involved in the phenomena of the stroboscopic discs. Several times also, instead of seeing the sector single, I noticed that it had a form approximating to that of the letter X or V, which evidently was due to the circumstance that almost involuntarily my eyes had glanced from one side of the disc to the other, thus giving rise to a combination of distinct visual impressions.

Next, for the purpose of examining the individual constituents of the flash, I placed on the terminal axis a circular disc, each quadrant containing a square opening of about 5°, and when this was revolving eleven times in a second, sometimes the flash was seen to consist of a small number (three to six) of apparently instantaneous discharges, the form of the square not being at all distorted. The same was afterward noticed with a velocity as high as 22 revolutions per second, and if the areas of the squares had been doubled this could not have escaped my attention, but as in many cases nothing of the kind was seen, it follows that the duration of the apparently instantaneous constituents was less than about $\frac{1}{1000}$ of a second.

On the other hand, sometimes the continuous duration of the constituents was such that the whole circumference of the disc was surrounded by a bright or faint ring of light, according to the original luminosity of the flash, which gave a duration for the continuous act at least as great as $\frac{1}{44}$ of a second. Quite often I was able to notice that these continuous discharges did not make up all the elements of the flash, but that they were terminated by an isolated and instantaneous discharge.

(5.) The last set of observations were made at Stockbridge, Mass., on the evening of August 25, 1872. The thunder was heard quite loudly and direct zig-zag flashes were occasionally seen; rain also fell. The disc employed on this occasion had only *one* square opening, the sides of which were seven millimeters (corresponding to about 12°), these dimensions having finally been found preferable. The rate of rotation was kept nearly constant by winding up.

The flashes were usually multiple, and the duration of the components was often or generally quite long, being as great as $\frac{1}{20}$ of a second if not longer; the brilliancy of the ring of light was considerable, and showed no signs of falling off throughout its whole extent. It was again noticed that when the duration of the earlier components had thus been considerable, the last act (or certain acts) were instantaneous. Eight or ten times it was fairly noticed that the components of certain flashes were to all appearance instantaneous, there being no distortion in the shape of the square. If its area had been increased by one half, this could not have escaped my attention, and would have implied a duration of $\frac{1}{300}$ of a second. A number of uncertain observations were made leading to a duration of the components, in some cases, of from $\frac{1}{300}$ to $\frac{1}{600}$ of a second, and finally in two cases the breadth of the square was distinctly doubled, giving a duration of about $\frac{1}{600}$ of a second.

To the above, I must add several observations made by the naked eye on *normal zig-zag* flashes, when on five or six occasions the duration of the direct flash was estimated at not less than one second, the light seeming to pour steadily in a stream from the cloud to the earth. They correspond to that of Faraday, referred to in the earlier part of this article.

It is evident from the foregoing that the nature of the lightening discharge is more complicated than has generally been supposed; it is usually, if not always, multiple in character, and the duration of the isolated constituents varies very much, ranging from intervals of time shorter than $\frac{1}{1000}$ of a second up to others at least as great as $\frac{1}{20}$ of a second, and furthermore, what is singular, a variety of this kind may sometimes be found in the components of a *single* flash. The long durations of certain constituents of the flash induced me afterward to make some experiments with a view of ascertaining the cause of this singular and unexpected phenomenon, and although the results are negative in character, still it may not be amiss briefly to mention them. I constructed a glass tube similar to those used for studying the spectral lines of rarefied gases, and having connected it with a mercurial air-pump, rarefied the ordinary air contained in it. A Leyden jar with a coating of 738 square centimeters was connected with the tube, and a spark micrometer was introduced into the circuit so as to interrupt it, and thus to cause the jar fairly to charge and discharge itself, as otherwise the prolonged discharges of the induction coil made their appearance. The tube was placed in front of the rotation apparatus used by me in investigating the electric spark, and its appearance studied at tensions from 1 millimeter upward, till the resistance became so great as to cut off the discharge. In every case the light appeared to be instantaneous, that is, its duration was so small as to preclude the idea of its affording an explanation of the long intervals occurring in the case of lightning flashes. Of course, in all these experiments, the actual rate of the mirror was quite low. I then repeated the same experiment with the vapor of water at a tension of about 15 millimeters with the same result.

Afterward a jet of steam was directed across the path of the electric spark in the free air, with a similar negative result. Finally, thinking that possibly the rain might in some way be concerned in the production of the prolonged durations, while sparks 10 to 15 millimeters in length, obtained from a smaller jar, were traversing the ordinary atmosphere, fine watery spray was directed across their path by the use of an "atomizer," without sensibly increasing their duration. These experiments may be explained by the greater volume of the atmospheric electricity, but suggest on the other hand, the possibility of a real prolongation of some of the constituents of the flash.

Recently the spectrum furnished by flashes of lightning has been examined by Dr. H. Vogel.* A number of lines were identified, as also occurring in the spectrum of the electric spark in the ordinary atmosphere, but what is remarkable, it was found that sometimes the spectra consisted of bright lines on a dark ground, while at others the bright lines were traced on a less bright continuous spectrum, and finally sometimes a continuous spectrum destitute of lines was obtained. The discharges were principally of the form known as sheet lightning. I consider it probable, that the continuous spectra destitute of lines observed by him, were due to the *prolonged* constituents mentioned in this paper, and the occurrence of bright lines on a less bright ground, I would refer to cases when instantaneous and prolonged constituents were mingled as noticed by myself; the normal spectrum of bright lines on a dark ground, finally, being produced by flashes more nearly instantaneous. Since writing the above I find that Wüllner in No. 11 of Pogg. Ann. for 1872, has shown that in rarified air, instantaneous sparks give a spectrum consisting of *lines*, while the prolonged constituent of the spark of an induction coil often produces a *banded* spectrum, which under certain circumstances approximates to one that is continuous.

It will be noticed, that while making observations No. 5, I was in the area occupied by the storm, in No. 4 on its outer edge, in No. 1 out beyond the edge, and in No. 3 the storm was quite distant. Yet in all these cases the character of the

* Pogg. Ann., 1871, No. 8, p. 653.

lightning, as far as could be observed, was quite identical, furnishing, as it seems to me, argument in support of the hypothesis that zig-zag lightning, heat and sheet lightning, etc., are really identical, being in point of fact due to the same cause, but viewed under different conditions.

Best form of apparatus for the study of Lightning Flashes.—From the contents of this article, it will be seen that additional observations are still desirable, and hence I wish to describe more definitely the apparatus which after many trials answered best, as well as to suggest one of a more perfect form. At the outset, I found it necessary to discard the plan of viewing a figure painted on an opaque disc, which seems to be the only one which had occurred to other physicists, its indications being uncertain, and the loss of light so great that it was impossible to observe even the *presence* of the less prominent constituents of the flash. It is advisable then to use a black or *grey* opaque disc, about 100 millimeters in diameter, with an *open* sector, and as has been shown in the present article, this is the only mode by which, thus far, actual measurements of the duration have been obtained. The multiple character of the flash renders it unadvisable to use more than one sector. The best form for the shortest and longest durations is that of a square, with sides of from seven to ten millimeters; for medium durations the same form can be retained with larger dimensions, and for examining the multiple character of the flashes, simply a long narrow sector of 1° or 2° is preferable. The use of the disc micrometer described by me in another place, I do not consider practicable with storms in our latitudes, though the frequency of the flashes in the Tropics might render possible its employment. A spring rotation apparatus, on account of portability, may be used, being so contrived as to admit of rates from one revolution up to 20 or 30 per second. This should be provided with some contrivance by which the observer might always be able to ascertain to what extent the clock-work at the moment had run down, so that the rate of the disc's rotation could afterward be found, without at the time interrupting the observations. A simple plan is to wind on the cylindrical portion of the key a string, which passes downward through the base of the instrument, knots being tied on it so as to indicate the number of turns already made by the slowest wheel.

The following is a more elaborate form with which more accurate results could be attained, and which probably would reveal details quite beyond the reach of the simpler apparatus. A circular disc of the kind just mentioned is to be set in rotation by clock-work run by a weight; an image of the disc of the natural size is to be formed on a vertical plate of ground glass by an achromatic lens of 7 or 8 inches focus, and of large aperture, (" portrait combination " from a photographic camera.)

An opaque circular screen is to be placed around the edge of the disc so as to cut off all stray light, which I found in my experiments very annoying. The observer placed behind the ground-glass will measure simply with a pair of compasses the length, etc. of the streaks, in the manner described by me in another place,[*] and the difficulty of accommodating the eyes for the image will in great part vanish, from the circumstance that the hands will be resting on the ground-glass where the images are expected. It is scarcely necessary to add, that to use this or any method successfully, will require some previous training, which I think could best be obtained by a repetition of the experiments and measurements described by me in a former number of this Journal.[*]

New York, Dec. 28th, 1872.

ART. XX.—*On the effects of Magnetization in changing the Dimensions of Iron, Steel and Bismuth bars; and in increasing the Interior Capacity of Hollow Iron Cylinders*; by ALFRED M. MAYER, Ph.D., Professor of Physics in the Stevens Institute of Technology.

PART I.

(Read before the National Academy of Sciences, in Cambridge, Nov. 22, 1872.)

I PURPOSE giving, in a series of papers, the results of a prolonged and careful research on the above subject.

Introduction.—In 1842 Joule discovered that when a current of electricity was passed through a helix which enclosed a bar of iron, the latter, on its magnetization, suddenly elongated a minute fraction of its length.

To present clearly Dr. Joule's experiments, we will give these abstracts from the excellent paper which he published in the Philosophical Magazine in 1847.

"In order to ascertain how far my opinion as to the invariability of the bulk of a bar of iron under magnetic influence was well founded, I devised the following apparatus. Ten copper wires, each 110 yards long and one twentieth of an inch in diameter, were bound together by tape so as to form a good, and at the same time very flexible conductor. The bundle of wires thus formed was coiled upon a glass tube 40 inches long and 1¼ inch in diameter. One end of the tube was hermetically sealed, and the other end was furnished with a glass stopper, which was itself perforated so as to admit of the insertion of a [illegible]illary tube. In making the experiments, a bar of annealed [illegible] one yard long and half an inch square, was placed in the ɔe, which was then filled up with water. The stopper was ɜn adjusted, and the capillary tube inserted so as to force the ter to a convenient height within it.

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[*] This Journal, III, vol. iv, Oct. 1872.

" The bulk of the iron was about 4,500,000 times the capacity each division of the graduated tube; consequently a very nute expansion of the former would have produced a very rceptible motion of the water in the capillary tube; but, on nnecting the coil with a Daniell's battery of five or six cells voltaic apparatus quite adequate to saturate the iron), no rceptible effect whatever was produced either in making or eaking contact with the battery, whether the water was sta- nary in the stem, or gradually rising or falling from a change temperature. Now had the usual increase of length been accompanied by a corresponding diminution of the diameter the bar, the water would have been forced through twenty visions of the capillary tube every time that contact was made th the battery.

"Having thus ascertained that the bulk of the bar was in- riable, I proceeded to repeat my first experiments with a ɔre delicate apparatus, in order, by a more careful investiga- n of the laws of the increment of length, to ascend to the obable cause of the phenomenon.

"A coiled glass tube, similar to that already described, was ed vertically in a wooden frame. Its length was such that ıen a bar one yard long was introduced so as to rest on the ıled end, each extremity of the bar was a full inch within the rresponding extremity of the coil. The apparatus for observ- the increment of length consisted of two levers of the first ler, and a powerful microscope situated at the extremity of e second lever. These levers were furnished with brass knife- ges resting upon glass. The connection between the free tremity of the bar of iron and the first lever, and that be- een the two levers, was established by means of exceedingly e platinum wires.

"The first lever multiplied the motion of the extremity of bar 7·8 times, the second multiplied the motion of the first imes, and the microscope was furnished with a micrometer vided into parts, each corresponding to $\frac{1}{3320}$ of an inch. nsequently, each division of the micrometer passed over by index indicated an increment of the length of the bar ıounting to $\frac{1}{138528}$th of an inch.

"The quantities of electricity passing through the coil were easured by an accurate galvanometer of tangents, consisting a circle of thick copper wire one foot in diameter, and a edle half an inch long furnished with a suitable index.

"The quantities of magnetic polarity communicated to the n bar were measured by a finely suspended magnet 18 inches long, placed at the distance of one foot from the center of the coil. This magnetic bar was furnished with scales precisely in the manner of an ordinary balance, and the weight required to bring it to a horizontal position indicated the intensity of the magnetism of the iron bar under examination.

"After a few preliminary trials, a great advantage was found to result from filling the tube with water. The effect of the water was, as De la Rive had already remarked, to prevent the sound. It also checked the oscillations of the index, and had the important effect of preventing any considerable irregularities in the temperature of the bar.

"The first experiment which I shall record was made with a bar consisting of two pieces of well-annealed rectangular iron wire, each one yard long, a quarter of an inch broad, and about one-eighth of an inch thick. The pieces were fastened together so as to form a bar of nearly a quarter of an inch square. The coil was placed in connection with a single constant cell, the resistance being further increased by the addition of a few feet of fine wire. The instant that the circuit was closed, the index passed over one division of the micrometer. The needle of the galvanometer was then observed to stand at 7° 20′, while the magnetic balance required 0·52 of a grain to bring it to an equilibrium. It had been found by proper experiments that a current of 7° 20′ passing through the coil was itself capable of exerting a force of 0·03 of a grain upon the balance; consequently the magnetic intensity of the bar was represented by 0·49 of a grain. On breaking the circuit, the index was observed to retire 0·3 of a division, leaving a permanent elongation of 0·7, and a permanent polarity of 0·42 of a grain. More powerful currents were now passed through the coil, and the observations repeated as before, with the results tabulated below.

"EXPERIMENT I.

Deflection of Galvanometer.	Tangent of deflection.	Elongation or shortening of bar.	Total elongation.	Magnetic intensity of bar.	Square of magnetic intensity divided by total elongation.
− 7° 20′	128	1·0 E.	1·0	−0·49	240
0	0	0·3 S.	0·7	−0·42	252
− 9 30	167	2·9 E.	3·6	−0·93	240
0	0	1·2 S.	2·4	−0·74	228
−14 48	264	5·9 E.	8·3	−1·42	243
0	0	3·8 S.	4·5	−1·00	222
−23 10	428	10·3 E.	14·8	−1·87	236
0	0	7·6 S.	7·2	−1·26	220
−47 25	1088	16·1 E.	23·3	−2·22	211
0	0	13·9 S.	9·4	−1·35	194
−58 50	1653	14·8 E.	24·2	−2·21	202
0	0	13·3 S.	10·9	−1·35	168

Dr. Joule now reversed the current in the helix and found that a current which deflected the needle 6° 15′ *shortened* the bar 3·4 div., and that after the current was broken its magnetic intensity was found reduced from −1·3 (the permanent intensity previously given by 47° 25′, see preceding table) to −17. He then passed a current of 9° 55′, and this he found was sufficient, not only to remove the former minus polarity of the bar, but also to give it a permanent polarity of +·25, and yet to leave the bar with 6·6 of the elongation belonging to its previous minus polarity.

Taking Joule's observations while the current was passing around the bar, we have for the current of 6° 15′ a magnetic intensity of −0·12, and for the current of 9° 15′ a *plus* magnetic polarity of 0·57. We call attention to these results because subsequent experimenters* seem to be unaware of these observations of Dr. Joule, who here first shows that a feeble current will demagnetize and even reverse the polarity of a bar which has previously required a far more powerful current to give it its permanent magnetic charge. In the experiment given above, the ratio of the current intensities of permanent magnetization and of demagnetization is 1088 to 175.

Dr. Joule now successively replaced the above bar by two others and obtained with them similar results. He then deduces the following important law. "From the last column of each of the preceding tables we may, I think, safely infer that *the elongation is in the duplicate ratio of the magnetic intensity of the bar*, both when the magnetism is maintained by the influence of the coil, and in the case of the permanent magnetism after the current has been cut off. The discrepancies observable will, I think, be satisfactorily accounted for when we consider the nature of the magnetic actions taking place. When a bar experiences the inductive influence of a coil traversed by an electrical current, the particles near its axis do not receive as much polarity as those near its surface, because the former have to withstand the opposing inductive influence of a greater number of magnetic particles than the latter. This phenomenon will be diminished in the extent of its manifestation with an increase of the electrical force, and will finally disappear when the current is sufficiently powerful to saturate the iron. Again, when the iron, after having been magnetized by the coil, is abandoned to its own retentive powers by cutting off the electrical current, the magnetism of the interior particles will suffer a greater amount of deterioration than that of the exterior particles. The polarity of the former may indeed be sometimes actually reversed, as Dr. Scoresby found it to be in some extensive combinations of steel bars Now whenever such influences as the above occur, so as to make the different parts of the bar magnetic to a various extent, the elongation will necessarily bear a greater proportion to the square of the magnetic intensity measured by the balance than would otherwise be the case.

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* Wiedemann, Pogg. Ann., c, p. 235; also R. W. Wilson, "Demagnetization of Electro-Magnets," Amer. Journ. Sci., 3d series, vol. iii, p. 346.

"For similar causes the interior of the bar will in general receive the neutralization and reversion of its polarity before the exterior, and hence we see in the tables that there is a considerable elongation of the bar after the reversion of the current, even when the effect upon the balance has become imperceptible, owing to the opposite effects of the interior and exterior magnetic particles."

Joule now experimented on a bar of unannealed iron, and on three bars of soft steel. As these bars had considerable degrees of retentive power, the anomalies occasioned by the above described actions did not exist to any considerable extent, and they gave a confirmation of the law that the elongation is proportional, in a given bar, to the square of the magnetic intensity.

The next bar he experimented with was of moderately hardened steel. This bar was slightly increased in length every time that contact with the battery was broken, although a considerable diminution of the magnetism of the bar took place at the same time. He says: "I am disposed to attribute this effect to the state of tension in the hardened steel, for I find that soft iron wire presents a similar anomaly when stretched tightly."

In a subsequent communication, contained in the same volume of the Philosophical Magazine, Dr. Joule gives accounts of numerous experiments made upon wires and bars of soft iron, cast iron, soft and hardened steel, subjected to various pressures and tensions while they were magnetized. As an example of the effect of *tension* on the phenomena, he states that in the case of a bar one foot long and a quarter of an inch in diameter, a tensile force of about 600 pounds caused all the phenomena of changes of length to disappear, even with a current which produced a deflection of 58° in the needle of the tangent galvanometer; but when a current of 61° was passed around this bar, subjected to a tension of 1040 pounds, it *shortened* 2·8 div. With a tension of 1680, and the same current, the bar shortened 4·5 divisions. Joule, from his experiments, deduces this law, viz: *In the case of tension the shortening effect is proportional to the current traversing the coil multiplied by the magnetic intensity of the bar.* He further states that "it is extremely probable that the shortening effects are proportional *cœteris paribus* to the square root of the force of tension."

In the case of bars of cast iron he finds that their elongation is equal, if not superior, to those of soft iron, when magnetized to the same degree; and an increase of tension in them does not produce half the retraction which is caused in soft iron bars in similar circumstances.

Bars of soft steel acted like the bars of iron, but the superior retentive powers of the former enabled him to trace better the elongating effects of the permanent magnetism, which diminished with the increase of tension and at last disappeared altogether; but with bars of perfectly hardened steel no sensible change in their lengths was produced by charges of *permanent* magnetism, and the *temporary* shortening effect of the coil was proportional to the magnetism multiplied by the current traversing the coil. The shortening effect did not, in these cases, sensibly increase with the increase of tension.

On subjecting bars of wrought and cast iron and soft steel to pressure, Joule found that it had no sensible effect upon the extent of their elongation. A hard steel cylinder a foot long, when submitted to the same experiments, with a pressure of 80 pounds, "suffered a diminution of length equal to 0·1 of a division of the micrometer, with a current capable of giving a magnetic polarity of 1·7."

At the termination of his paper, Dr. Joule gives the following "*postscript.*" "I have already, in the former part of this paper, described an experiment which indicated that no alteration in the *bulk* of a bar of soft iron could be produced on magnetizing it. I thought, however, that it would be interesting to confirm the fact by an observation of the alteration of the dimensions of the iron at right angles to the direction of its polarity. For this purpose I took a piece of drawn iron gas-piping one yard long, $\frac{3}{16}$ths of an inch in bore, and $\frac{3}{16}$ths of an inch in thickness. A piece of thick, covered copper wire was inserted into this tube, and bent over the outside of it. The lower extremity of the iron tube being fixed, and the upper end being attached to the micrometrical apparatus, each division of which corresponded to $\frac{1}{136528}$ths of an inch, I obtained * * * results which show that the length of the tube was diminished, in order to make up for the increase of its diameter, which, in this instance, was in the direction of the polarity. The quantity of the shortening effect, viz: 3·4, is, however, only one-third of that due to the maximum elongation of soft iron bars as observed in the first section. This is probably owing to the grain of the iron being in cross directions with respect to the polarity in the two cases; and partly, perhaps, to the iron tube not being fully saturated with magnetism. The experiment is worth repeating, especially as it affords a means of studying the magnetic condition of closed circuits."

Remarking on the cause of the phenomena of elongation, Dr. Joule says: "The law of *elongation* naturally suggests the joint operation of the attractive and repulsive forces of the constituent particles of the magnet as the cause of the phenomena. On the other hand, the fact that the *shortening effect* is proportional to the magnetic intensity of the bar multiplied by the current traversing the coil, seems to indicate that, in this case, the effect is produced by the attraction of the magnetic particles by the coil. But then it will be asked, why so remarkable an augmentation of the effect is produced by the increase of tension in the case of the soft iron bars? When we are able to answer this question in a satisfactory manner, we shall probably have a much more complete acquaintance with the real nature of magnetism than we at present possess."

This full account of Dr. Joule's remarkable research is here presented in order to give an exposition of our present knowledge of this subject, and clearly to set forth the relations which my own attempts bear to his labors. Here Joule, the discoverer of these phenomena, has given us almost all the knowledge we have, up to this time, possessed in reference to their characteristics and their laws. That a subject so fascinating should not have been eagerly followed up appears strange; especially so, when it seems highly probable that the faithful study of these actions may one day give us an insight into the dynamic nature of electro-magnetization and thus lead the investigator into a fruitful field of research.

No one can duly appreciate this work of Joule's until he attempts the confirmation of his results; then the difficulties of the research and the skill and acumen of this eminent physicist will be properly estimated.

Although the cognate discovery by our countryman Page, in 1837, that iron bars produce sound on their magnetization, has been carefully studied by Delezenne, De la Rive, Beatson, Marrian, and Wertheim, yet in the annals of science I have found only two experimental investigations, in addition to the one by Joule, on the phenomena of the elongation produced in iron rods on their magnetization. The first is by Wertheim, in the Ann. de Ch. et de Phys., 3e Serie, t. xxiii; the second by Tyndall, contained in a paper entitled "On some Mechanical Effects of Magnetization;" published in his "Researches on Diamagnetism and Magne-Crystallic Action," London, 1870.

In Wertheim's memoir "On the sounds produced in Magnetized Iron," all we find on the subject of the elongation of magnetized iron rods is the following: "Here are the results of these experiments: the helix being placed so that its axis coincides with that of the bar, we do not observe any lateral movement, but only a very small elongation; this elongation rarely surpasses ·002 millimeter, [in rods about 970 millimeters long] and although visible is hearly measurable; it is most pronounced when the helix [whose length was a little over ⅓th of that of the rod] encloses the extremity of the bar; it diminishes as the helix approaches the point [the center] where the rod is clamped, and it is probable that when it is quite close to this point, the elongation changes into a retraction, but I have never been able to observe the motion in this direction with any certainty. * * * * * I have already remarked that it was not possible for me to measure this longitudinal traction; happily Mr. Joule has supplied that omission."

Dr. Tyndall opens his paper thus: "Wishing, in 1855, to make the comparison of magnetic and diamagnetic phenomena as thorough as possible, I sought to determine whether the act of magnetization produces any change of dimensions in the case of bismuth, as it is known to do in the case of iron. The action, if any, was sure to be infinitessimal, and I therefore cast about for a means of magnifying it. * * * * * I consulted Mr. Becker, and thanks to his great intelligence and refined skill, I became the possessor of the apparatus now to be described. * * * * * The same apparatus has been employed in the examination of bismuth bars; and, though considerable power has been applied, I have hitherto failed to produce any sensible effect. It was at least conceivable that complimentary effects might be here exhibited, and a new antithesis thus established between magnetism and diamagnetism."

The apparatus used by Dr. Tyndall consisted of two vertical brass rods firmly cemented into a block of stone. Between these rods, securely fixed in the stone, were placed the rods of iron whose elongation he desired to measure. On the vertical rods slid a transverse bar of brass carrying "a vertical rod of brass, which moves freely and accurately in a long brass collar. The lower end of the brass rod rests upon the upper flat surface of the iron bar. To the top of the brass rod is attached a point of steel; and this point passes against a plate of agate, near a pivot which forms the fulcrum of a lever. The distant end of the lever is connected by a very fine wire, with an axis on which is fixed a small circular mirror. If the steel point be pushed up against the agate plate, the end of the lever is raised; the axis is thereby caused to turn, and the mirror rotates." The angular deflections of the mirror he determined by the method of Poggendorff; that is, by viewing in a telescope the divisions of a fixed scale reflected from the mirror.

Dr. Tyndall gives the following account of his experience with this apparatus. "Biot found it impossible to work at his experiments on sound during the day in Paris; he was obliged to wait for the stillness of night. I found it almost equally difficult to make accurate experiments, requiring the telescope and scale, with the instrument just described, in London. Take a single experiment in illustration. The mirror was fixed so as to cause the cross hair of the telescope to cut the number 727 on the scale; a cab passed while I was observing—the mirror quivered, obliterating the distinctness of the figure, and the scale slid apparently through the field of view, and became stationary at 694. I went up stairs for a book; a cab passed, and on my return I found the cross hair at 686. A heavy wagon then passed, and shook the scale down to 420. Several carriages passed subsequently; the figure on the scale was afterward 350. In fact, so sensitive is the instrument, that long before the sound of a cab is heard, its approach is heralded by the quivering of the figures on the scale.

"Various alterations which were suggested by the experiments were carried out by Mr. Becker, and the longer I worked with it the more mastery I obtained over it; but I did not work with it sufficiently long to perfect its arrangement. Some of the results, however, may be stated here.

	Figure of Scale.
Bar unmagnetized,	577
" magnetized,	470
" unmagnetized.	517

"Here the magnetization of the bar produced an elongation expressed by 107 divisions of the scale, while the interruption of the circuit produced only a shrinking of 47 divisions. There was a tendency on the part of the bar, or of the mirror, to persist in the condition superinduced by the magnetism. The passing of a cab in this instance caused the scale to move from 517 to 534—that is, it made the shrinking 64 instead of 47. Tapping the bar produced the same effect.

"The bar employed here was a wrought iron square core, 1·2 inch a side and 2 feet long.

"The following tables will sufficiently illustrate the performance of the instrument in its present condition. In each case are given the figures observed before closing, after closing, and after interrupting the circuit. Attached to each table, also, are the lengthening produced by magnetizing and the shortening consequent on the interruption of the circuit:—

Circuit.	Scale 10 cells.		Circuit.	Scale 20 cells.	
Open,	647		Open,	653	
Closed,	516	131 elongation.	Closed,	475	188 elongation.
Broken,	581	65 return.	Broken,	579	114 return.
Open,	637		Open,	638	
Closed.	509	128 elongation.	Closed,	452	186 elongation.
Broken,	579	70 return.	Broken,	568	116 return.
Open,	632		Open,	632	
Closed,	491	141 elongation.	Closed,	472	160 elongation.
Broken,	568	77 return.	Broken,	561	89 return.

"These constitute but a small fraction of the numbers of experiments actually made. There are very decided indications that the amount of elongation depends on the molecular condition of the bar.* For example, a bar taken from a mass used in the manufacture of a great gun at the Mersey Ironworks, suffered changes on magnetization and demagnetization considerably less than those recorded here. I hope to return to the subject."

That the tilt of the mirror in this instrument should be controled alone by the molecular motions of the bar, was hardly to be expected from a critical examination of the construction of the apparatus. The sudden upward push on the fulcrum might readily cause a minute permanent displacement of this delicate axis, and this change of position being greatly magnified by the lever and mirror would affect considerably the mirror's subsequent position of repose. Also, if on the above sudden and powerful impulse the long arm of the lever received a slight permanent flexure, or if the slender wire, connecting the end of this lever-arm with the mirror-axis, should become slightly elongated, these strains would determine a change of position in the mirror after contact with the battery was broken. The marked want of stability in the scale readings I attribute to the fact, that the weight of the indicating apparatus was placed on long brass rods, whose upper ends were unrestrained from partaking of tremors whose pulses were synchonous with the time of vibration of its system of weighted rods. These tremors transmitted though the ground to them caused the apparatus to partake of the nature of an instrument known to astronomers as "Hardy's Noddy," which is a species of inverted pendulum, and was even used by Captain Kater to detect any vibrations in the support of the pendulum used in his celebrated "Experiments for determining the Length of the Pendulum vibrating Seconds" (Phil. Trans., 1818, p. 42).

ART. XXI.—*Investigations on Parasulphobenzoic Acid;* by IRA REMSEN.

A SHORT time ago I commenced a series of investigations the object of which was to throw additional light upon the subject of "molecular rearrangement in aromatic compounds." I was guided by the hope that many, if not all, of those cases of apparent anomaly, which had been referred to molecular rearrangement, might, by a careful reëxamination, find other and more satisfactory explanations: and that this inexplicable and dangerous bugbear might, in consequence, be forced at least from that division of organic chemistry which comprises the so-called aromatic compounds. Up to the present the object in view has hardly been approached, inasmuch as attractive byways were soon disclosed, and these were followed. Leaving then for a future paper the consideration of the subject mentioned above, until its study shall have become more complete, I shall here give a description of one of the byways which drew me away from the main road.

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* This fact had previously been shown by Joule's experiments.

I. *Paraoxybenzoic Acid from Sulphobenzoic Acid.*

A most glaring case of apparent molecular rearrangement is that noticed in connection with the formation of protocatechuic acid from oxybenzoic and paraoxybenzoic acids; and the formation of pyrocatechin (and only this) by the dry distillation of protocatechuic acid. To this case I have already referred,[*] and it was, indeed, for a short time the subject of a discussion[†] which ended temporarily in an unsatisfactory manner. I hope by its further study to be able to come to a definite conclusion in regard to it. My attention was at first directed to the study of the oxybenzoic acid from which the protocatechuic acid had been prepared. Adopting the method which had already been employed for the preparation of this acid, I converted a quantity of benzoic acid into sulphobenzoic acid, and melted the potassium salt of this latter with potassium hydroxide. On extracting the product in the usual manner, by means of ether, and crystallizing it from water, I was astonished to find that, on cooling, well-formed, slightly colored crystals were deposited from the solution, instead of the crystalline mass or verrucous masses characteristic of oxybenzoic acid, which I anticipated. The general appearance of these crystals led immediately to the conclusion that in this case paraoxybenzoic acid had been formed; and further examination showed this to be true. The substance was recrystallized from water, and then possessed all the characteristics of a pure chemical compound. The crystals were now colorless and sharply defined in form. They were transparent; but, on being heated to 100° in an air-bath, they became opaque, the presence of water of crystallization being thus indicated. The fusing point was found to be 210° in repeated experiments; and decomposition took place when the crystals were heated slightly above the fusing point. The point of solidification was 160°. All these properties taken together prove that the substance under consideration is not oxybenzoic acid; and the proof that it is paraoxybenzoic acid was rendered complete by the analysis, which gave the following results:

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[*] Zeitschrift für Chemie, N. F. 7, 294.

[†] See Barth, Berliner Berichte, iv, Jahrgang, S. 633; Ascher, ibid, iv Jahrgang. S. 650; Fittig, Zeitschrift für Chemie, N. F. 7, 181.

I. 0·3308 grams substance, heated to 110°, lost 0·0383 grams H^2O.
II. 0·2925 grams dried substance gave 0·6502 grams CO^2 = 0·17733 grams C, and 0·1181 grams H^2O =0·01312 grams H.

	Calculated.		Found.
C^7	84	53·85	53·61
H^6	6	3·84	3·97
O^3	48	30·77	....
H^2O	18	11·54	11·58
	156	100·00	

Paraoxybenzoic acid, then, according to this, can be the product of the action of potassium hydroxide on sulphobenzoic acid. But this reaction has been employed for the preparation of pure oxybenzoic acid,—first by Barth,* and afterward by Heintz †—and, as neither of these chemists mention the formation of paraoxybenzoic acid under these circumstances, it is fair to suppose that, if formed, it escaped their attention: that it could not have made its appearance in such quantities as in the experiment described, is evident. The question now naturally arose whether the conditions under which I had prepared the sulphobenzoic acid had been of influence on the product. Instead of waiting until the sulphuric anhydride had entirely dissolved the benzoic acid, as Barth did, I had added a small amount ot fuming sulphuric acid to the semi-liquid mass after a time, and then heated gently until complete solution resulted. This shortened the operation somewhat, as the lumps of benzoic acid, which had become packed together, were thus brought under the direct influence of the fuming acid; whereas, before, they were covered with a thick pasty layer which protected them.

In order to decide the question I made two experiments. First, sulphobenzoic acid was prepared strictly according to the method of Barth, every letter of his directions being followed, and the acid thus prepared melted with potassium hydroxide. Second, sulphobenzoic acid was prepared by simply heating benzoic acid with fuming sulphuric acid; and the product melted with potassium hydroxide. Strange to say, these two experiments gave identical results, which differed from that already obtained. Paraoxybenzoic acid was indeed formed in both cases, but in much smaller quantity than in the first experiment. Instead of appearing in the first deposit of crystals, it was now not observed until the second or third; but then unmistakably. It possessed all the characteristics of the acid—its crystalline form, fusing point, water of crystallization, etc.

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* Annalen der Chemie und Pharmacie, cxlviii, 30.

† Ibid., cliii, 326.

After this a large number of experiments were made, with the object of discovering the conditions which are favorable to the production of paraoxybenzoic acid by means of this reaction; but, although this acid was in every case obtained as a secondary product in much smaller quantity than oxybenzoic acid, the greatest possible variations failed to bring about the first remarkable result. I was hence obliged to abandon the hope of clearing up this point, and to look upon the innumerable experiments as having increased rather than decreased the mystery. It is thus, at least, shown that the product, which had been looked upon as an individual substance, is in reality a mixture; and, inasmuch as the properties of oxybenzoic acid are such as to preclude the possibility of judging positively in regard to its purity from its appearance; and, as its thorough separation from paraoxybenzoic acid by means of crystallization, when the two are present in the mixture in anything like equal proportions, is an impossibility; I endeavored to discover some other means which might be employed advantageously for the purpose of separation.

It seemed possible, from a study of the salts of the two acids, that the cadmium salt might be called in to aid successfully in this project. But experiment soon showed that, however well the cadmium salt of paraoxybenzoic acid might crystallize in a pure condition, it resisted all attempts when mixed with the oxybenzoate.

According to Barth, the basic barium salt of paraoxybenzoic acid is easily formed by the addition of an excess of barium hydroxide to a somewhat concentrated solution of the acid; and this salt, being insoluble, or nearly so, is precipitated under these circumstances, whereas no corresponding salt of oxybenzoic acid is formed. Taking advantage of this fact, the solution of the two acids was treated with barium hydroxide, and a precipitate was thus obtained. On decomposing this precipitate a quantity of pure paraoxybenzoic acid was obtained; but on examining that which still remained in solution it was found to be a mixture, and this method of separation proved to be of no value. Since the publication of my first notice on this subject, Barth has repeated his former experiments, and confirms my statement that paraoxybenzoic acid is always formed in the preparation of oxybenzoic acid from crude potassium sulphobenzoate. He, at the same time, however, remarks that pure oxybenzoic acid may be obtained by means of this reaction, it being merely necessary to recrystallize the crude product a few times from water. It is very possible that a pure substance may be obtained in this way, but it is a difficult matter to prove positively that this is the case, especially as we know that there is a source of impurity which, as we have seen, may vary greatly in its influence upon the character of the product. It seems to me then that, whenever for test experiments pure oxybenzoic acid is required, it would be advisable to become convinced of its purity by some other means; and I would suggest the preparation of pure acid barium sulphobenzoate as the first step. This salt can be readily obtained in beautiful, perfectly formed crystals, the appearance of which is a test of their purity; and from this *pure* oxybenzoic acid can be obtained.

II. *Parasulphobenzoic Acid a constituent of crude Sulphobenzoic Acid.*

The formation of paraoxybenzoic acid under the circumstances mentioned above might be due to two causes. Either metasulphobenzoic acid might yield it through the instrumentality of molecular rearrangement under the influence of heat and fusing potassium hydroxide, or crude sulphobenzoic acid, prepared as above, might contain both the para- and meta-varieties, which would account for the complex character of the resulting oxyacid.

To test the first possibility, a quantity of pure acid barium metasulphobenzoate was prepared, and then converted into the potassium salt. This was melted with potassium hydroxide, and the product carefully examined. Not a trace of paraoxybenzoic could be discovered in it. The experiment was repeated a number of times, but the result was invariable. The second possibility thus became more probable.

That portion of crude sulphobenzoic acid which had yielded the large proportion of paraoxybenzoic acid was now investigated, in order if possible to detect the presence of a second substance in it. It was neutralized with pure barium carbonate, and the excess of the latter and the precipitated sulphate then filtered off. The clear solution was separated into two equal portions and from one of these the barium precipitated by pure sulphuric acid, care being taken to avoid the least excess of the latter. On now mixing the two clear solutions again, evaporating to the point of crystallization, and allowing to cool, long, flat, acicular crystals made their appearance. These had no resemblance to the known acid barium salt of sulphobenzoic acid. They were recrystallized from water, and were now obtained in exceedingly beautiful form. Repeated recrystallizations failed to change this form. This being established, the salt was analyzed with the following results:

I. 0·525 grams salt lost in weight constantly up to 200°; above this temperature no loss took place. The entire loss was 0·047 grams. This portion of the salt gave 0·206 grams $BaSO^4 =$ 0·12113 grams Ba.

II. 0·5307 grams salt lost 0·494 grams at 200°; and gave 0·2078 grams $BaSO^4$=0·12219 grams Ba.

	Calculated.		Found.	
$(C^{14}H^{10}S^2O^{10})$	402	67·79		
Ba	137	23·10	23·07	23·02
$3HO^2$	54	9·11	8·95	9·31
	593	100·00		

The formula thus deduced, viz: $(C^7H^5SO^5)^2Ba + 3H^2O$, is, however, the same as that given for the known salt of sulphobenzoic acid; and hence, though the evidence might be strong in favor of considering the analyzed crystals as representing a second and new variety of the salt, it was by no means conclusive. Two experiments were now made, the results of which were decisive. In the first place the mother-liquor from the salt obtained was evaporated down, and then yielded a mixture of two well characterized salts, the long, flat crystals and moderately well formed, apparently monoclinic prisms. On separating the latter as well as possible from the superimposed crystals, and recrystallizing them, they were soon very much improved in appearance, being now perfect in form, and corresponding in every way to the known salt of sulphobenzoic acid. These, as well as the other crystals mentioned, retained their form through a series of crystallizations. They were analyzed for the purpose of comparison.

0·5182 grams salt lost 0·0484 grams at 200°; and gave 0·2038 grams $BaSO^4$=0·1198 grams Ba.

	Calculated.		Found.
$(C^{14}H^{10}S^2O^{10})$	402	67·79	
Ba	137	23·10	23·12
$3H^2O$	54	9·11	9·34
	593	100·00	

Again, the acicular crystals were converted into the potassium salt, and this melted with potassium hydroxide. The reaction was very clean, yielding immediately a perfectly colorless, well crystallized product. This possessed all the characteristics of paraoxybenzoic acid, viz: the same degree of solubility in water, the same fusing point, the same crystalline form; they also contained water of crystallization. With these facts an analysis was deemed unnecessary. The conclusion is, therefore, justified that the acicular crystals are the salt of a second variety of sulphobenzoic acid, which, adopting the ordinary nomenclature, would naturally be called *parasulphobenzoic acid.*

The presence of this acid in the crude product from the action of sulphuric acid in benzoic acid accounts then satisfactorily for the unexpected formation of paraoxybenzoic acid under the circumstances mentioned above. The varying proportions of oxybenzoic and paraoxybenzoic acids, as final products of the series of reactions, corresponded to similar varying proportions of metasulphobenzoic and parasulphobenzoic acids formed in the primary reaction. This simultaneous formation of the two sulpho-acids is in perfect harmony with known facts, it being the rule that, when substitution-products are formed by the direct action of subtituting agents upon the mother-substance, at least two varieties of the product are formed, if this is possible. But why is it that in one case a larger (sometimes very large) quantity of the para-acid is formed, while in another only a small quantity is formed? I have in vain endeavored to answer this question; and it was with a feeling of great dissatisfaction that I was obliged to abandon it, as the solution of this problem would have had a much greater interest than the discovery of the parasulphobenzoic acid. It appeared probable that the case under consideration might correspond to that studied by Kekulé* in connection with the isomeric varieties of sulphophenolic acid. In this case the meta-acid is formed almost exclusively when the reaction is allowed to take place at the ordinary temperature, but when the temperature is elevated the amount of the para-acid is increased gradually, until finally it is the only product. The meta-acid is then converted into the para-acid by the action of heat and sulphuric acid. Acting according to the suggestion thus offered, I attempted to convert metasulphobenzoic acid into the para-variety; but, as already stated, no amount of heating, no matter how long continued, brought about the desired result. Variations in the method of preparation were introduced as long as thought continued to suggest them; some of these were apparently trivial, some decided; but I only succeeded in accumulating a mass of negative imformation of no particular value.

According to the experiments made, parasulphobenzoic acid is always produced when sulphuric acid acts upon benzoic acid. When it is present in the product in comparatively large quantity, it can be readily separated from the isomeric compound by means of the acid barium salt. If, however, it is present in small quantity, as is generally the case, it is very difficult, if not impossible, to obtain it in a pure condition. On preparing the acid barium salt, and evaporating the solution of the mixture, pure meta-salt is at first deposited; but when the solution has attained a certain degree of concentration the two salts crystallize out together, the long crystals generally appearing first and the monoclinic crystals of the meta-salt being then deposited upon and among these in such a complicated manner that the task of separating the two mechanically is not in the least attractive nor promising. Repeated crystallizations do not change the character of the crystals.

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* Berliner Berichte, ii Jahrgang, S. 330.

I now endeavored to find other means of separation; and among those tested was the partial crystallization of the acid sodium salts of the two acids. No better success attended this experiment. All other experiments made in this direction gave the same result. I was thus prevented from gaining possession of any respectable quantity of the new acid in this way; and as its study seemed to offer a prospect of interesting results, I turned my attention to attempts to find other methods for its preparation.

[To be continued.]

ART. XXII.—*On Dynamical Theories of Heat;* by Prof. W. A. NORTON.

QUERY. *Is heat any mode of motion of the atoms of ordinary matter; such atoms being regarded, in accordance with the common notion of an atom, as incapable of experiencing any change of form or dimensions, or in the intensities of their acting forces?* There are three conceivable modes of motion of such invariable atoms—a vibratory or oscillatory motion, either rectilinear or curvilinear; a revolution of one atom around another; or a rotation of each about an axis.

Let it be distinctly understood that *the inquiry has reference only to atoms of ordinary or gross matter.* There can be no question that heat, in its origin, and generally in its manifestations within ordinary bodies, consists in some form of periodic movement, attended with regularly recurring impulses communicated to the ether which fills all space and pervades the interstices of bodies; since waves of radiant heat cannot possibly have any other origin. But the question is, whether the constituent atoms of bodies have this movement, or those of some form of ethereal matter intimately associated with these atoms.

I. *Can heat be any mode of vibratory, or oscillatory motion, of the atoms of gross matter.* Against this notion of the origin, or nature of heat, many serious objections may be urged.

1. *It implies rates of vibration inconceivably more rapid than we have any independent reason to suppose can take place in the interior of bodies.* The most rapid molecular vibratory motion that we actually know of, occurs when a vibrating body emits a musical sound of the highest pitch that the ear is capable of detecting. This is at the rate of less than 80,000 vibrations per second, but an atom emitting the heat-rays of the red end of the

spectrum vibrates at the astonishing rate of 458 millions of million times per second. Thus in the supposed heat motion the vibration is six thousand million times more rapid than in the most acute audible musical sound. It is true the disproportion between the quantities of matter in vibration in the two cases may be very great, but it does not follow that the rate of vibration should augment in the same proportion. The case is not analogous to that of vibrating strings of diverse lengths. The comparative rapidity of movement in the two cases must depend on the comparative intensities of the accelerating forces, atomic and molecular, in operation. That the astonishing disproportion above stated, between the velocities of vibration, answering to the heat-rays and to the most acute musical sound, may exist, it is necessary that the same enormous disproportion should subsist between the accelerating forces in operation.

2. *The hypothesis under consideration implies, then, that the atomic forces in operation in the production of heat and light are inconceivably more intense than the molecular forces in action when musical sounds are originated, or propagated.* That this may be true, it is necessary, 1st, that in the case of every substance, whether in the solid, liquid, or gaseous state, the aggregation should be that of molecules made up of atoms, never that of simple atoms; since if this were not the case the forces in play in the origination of atomic heat-vibrations, would be identical with those in operation in the origination and propagation of sound. 2d, that in every instance the distance between the molecules should be vastly greater than that between their constituent atoms. Now since the mutual action of two contiguous molecules is the result of the joint action of all their atoms, and since in the case of a solid, at least, it cannot be doubted that the contiguous molecules are in a state of equilibrium under the mutual action of their own forces alone (or very nearly so), their distance apart must be that at which all the mutual attractive actions exerted by the more distant atoms of the two groups is neutralized by the repulsions which take effect between those that are nearest to each other. The distance between the surfaces of the two molecules cannot then, in general, be materially different from that between their constituent atoms. It should be less rather than greater. As to the distance between the centers of contiguous molecules, it cannot be many times greater than that between their atoms, unless the number of atoms combined together in each molecule is very great. When a substance is in the liquid form, the distance between the molecules cannot be materially different, from our present point of view, from that which obtains in the solid form; if, in fact, the aggregation of a simple substance in the liquid form be not that of atoms simply, instead of compound molecules.

In the propagation, then, of sound, or a force of percussion, through a solid or liquid, the force of repulsion developed between the contiguous atoms of two molecules should be as great as would be developed between the constituent atoms of the molecules by an equal relative displacement. The velocity imparted by this repulsion to the molecule, would of course be less than that communicated to the single atom by an equal force; but less only in the ratio of the number of atoms in the molecule to unity. Unless, then, the integrant molecules of an elastic solid, or of a liquid, contain a large number of atoms the velocity of propagation through it of sound, or a force of percussion, should not be many times less than that which should occur from atom to atom of each of its molecules, under the operation of atomic forces conceived to be of sufficient intensity to impart hundreds of thousands of millions of millions of vibrations in a second—a velocity of propagation which should be comparable with that of light itself, by the luminiferous ether. We may conclude, therefore, that the immense disproportion between the rates of movement, and accelerating forces in operation, in the origination and propagation of waves of heat and of sound, can only be reconciled with the theory of atomic heat-vibrations under consideration, by supposing that the integrant molecules of all bodies, solid or liquid, contain millions of atoms. It is hardly necessary to add that there are insuperable objections to this supposition, on both chemical and physical grounds.

3. *Certain phenomena attendant upon the development of heat by impact, are opposed to the hypothesis of atomic heat-vibrations.* It is found that a certain amount of heat is developed, as the final result of any case of impact, provided there is a residual compression of one or both of the impinging bodies at the close of the impact, not otherwise; and that the quantity of heat evolved is proportionate to the degree of condensation. Now this residual condensation implies that the impact has augmented the attractive forces exerted by each molecule upon contiguous molecules; the repulsive forces exerted by it, if experiencing any change, certainly not being proportionally increased. The constituent atoms of each molecule must then, if invariable as supposed, have taken up new positions of equilibrium, in which the attractive actions exerted by them upon the atoms of a contiguous molecule have augmented. Hence the actions of the constituent atoms of each molecule on one another must have changed. Now such a change in the intensities of the atomic forces that take effect at a given distance, cannot result from the mere fact of atomic vibrations. Again if we reflect that *the compression existing at the moment of the separation of the two colliding bodies, which serves as the meas-*

ure of the heat evolved, may have an indefinite series of values, varying by ever so small degrees with the force of impact, and with the degree of elasticity, we shall see that the molecules must be capable of taking up an endless number of new positions of equilibrium. But this is plainly impossible if the inherent atomic forces are invariable, since the mutual actions of the molecules, being the joint actions of their constituent atoms, cannot vary unless the individual atomic actions of which they are made up vary, and these cannot change, by such small degrees, unless the actions of the constituent atoms of each molecule on one another change; for it is in this way alone that these atoms can take up an indefinite series of new positions of equilibrium, and so the molecules be capable of exerting corresponding varieties of action on one another.

A similar conclusion may be arrived at, by considering the phenomena of the set of bars and beams, when relieved of strain. It has been established as the result of diverse series of experiments, under varied circumstances, that when a bar, or beam, is relieved of any strain to which it has been subjected by a weight, there is a small residual change of dimensions, form, or position, which is termed the *set*; and that this set, from being barely appreciable when the smaller weights are employed, steadily and slowly increases with the weight used. For example, according to the results of Hodgkinson's experiments on the compression of a bar of cast iron, 10 ft. long, confined in a vertical position between guides, the set remaining after a compressing weight of about 2,000 lbs. per square inch of section was removed, was only $\frac{1}{356,000}$ of the length—that is, the distance between contiguous molecules in the line of the bar was diminished by this fractional amount. The weights being gradually increased, from 2,000 lbs. to 33,000 lbs., the set remaining after their removal augmented by slow degrees from $\frac{1}{356,000}$ to $\frac{1}{3,000}$ of the length. The atoms of the individual molecules of the iron must then have taken up a corresponding number of slightly varying positions of equilibrium, giving rise to new intensities in the forces of action of one molecule on another at a given distance. Now such a series of slightly differing positions of equilibrium of the molecules, is plainly irreconcilable with any other hypothesis than that of the capability of variations, by indefinitely small degrees, in the intensities of the atomic forces exerted at given distances.

Whether, then, we consider the residual effects of a force of percussion, or of a force of pressure, we find that if we take the ground that the atomic forces vary in intensity only with the distance of action, we are involved in this dilemma; the effects observed cannot have place unless the constituent atoms of the molecules are capable of taking an indefinite number of slightly differing positions of equilibrium, and these diverse positions cannot be taken up if the forces exerted remain the same at the same distance. We are, therefore, compelled to abandon one prominent feature of the doctrine of atomic heat-vibrations, and admit that *the mutual actions of atoms are liable to change, under the influence of applied forces, by reason of some change taking place in the physical and internal mechanical condition of the atoms themselves.*

We may conclude, therefore, that when two bodies collide, the condensation that subsists at the close of the impact is, in all probability, due to some action, exerted by the force developed in the collision, upon the ultimate atoms of the bodies, which has the effect to change the physical and internal mechanical condition of each atom—a result which is conceivable only on the supposition that "the atom," so-called, *is made up of distinct parts capable of relative displacement.* We conclude also that the evolution of heat which accompanies the condensation, and is proportionate in amount to it, is probably due to the same physical change experienced by the atoms.

Let us now attempt to gain a nearer insight into the detail of the process of evolution of heat by impact It will be conceded, that in the collision of two bodies a force of mutual repulsion comes into operation between the molecules at the point of contact; which goes on increasing in intensity as the minute distance between these molecules diminishes, and then decreases to zero at the instant of the final separation of the bodies. It is this force of mutual repulsion, developed at the point of contact, that is the immediate operative cause of all the phenomena of the impact.* This force cannot be the repulsion of sensible heat, for the sensible heat-energy that pertains to any body is definite in amount, and if any portion of it is expended in the impact, it can, at most, only be given out again at the close, and hence the body could experience no increase of temperature. Now, in the impact of *perfectly inelastic bodies*, the fact that there is no recoil shows that no portion of the energy of the moving bodies, lost in the impact, is expended in effecting their final condensation; for if it were, the potential energy of the resistance that would be in operation at the moment when the bodies had a common velocity, would be expended in the production of a recoil. But this is equivalent to saying that the atoms and molecules take up, of themselves, by reason of variations in their mutual actions, in some way induced by the impact, a series of new virtual positions of equilibrium in the process of condensation. Again, if, as we have seen, no portion of the energy lost in the impact is expended in effecting the final condensation, then it follows that the whole of the energy lost is consumed in the production of some form and amount of movement which must be, either potentially or actually, the heat-energy developed by the impact. This movement must be associated with, and proceed *pari passu* with the changes of atomic and molecular actions above mentioned, and hence cannot be the supposed atomic heat-vibrations. But it may be conjectured that it may still be some potential change in the positions of the atoms, from which result atomic heat-vibrations. The answer to this is, that it is impossible that any such changes of position could form a series of positions of equilibrium of the molecules and atoms (which we have seen obtain, so far as the natural atomic forces are operative, in the case of inelastic impact) and at the same time result in atomic vibrations; since such vibrations imply that the mutual actions of the atoms, in the displaced positions, are either effectively attractive, or effectively repulsive, and hence these positions could not answer to states of equilibrium.

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* The mutual repulsion between the molecules in the interior of the two bodies comes into operation, it is true; but it is only incidentally, as an antagonistic force to the condensation which the repulsion developed between the contact-molecules tends to produce, in arresting the motion of approximation of the two bodies. The amount of the condensation experienced by the bodies, and the duration of the contact, will vary with the intensity of the internal elastic resistance thus called into play, for a given displacement of the molecules: or, in other words, with their coefficient of elasticity. They should also vary with any deviations from perfect elasticity that may subsist.

To examine the process more closely; at the instant when the distance between the molecules at the point of contact of the two bodies has reached its minimum, and, therefore, the mutual repulsion between them has augmented to its maximum value, the two bodies have been brought to a common velocity. In accomplishing this a certain amount of energy has been expended, which is measured by the mean value of the mutual repulsion between the contact-molecules during the minute interval of time that this repulsion has been in operation, multiplied by the minute distance that these molecules have approached each other during the same interval. This is the greatest amount of energy, or living force, that the bodies can possibly lose in the impact. At the instant supposed, the condition of maximum approximation of the contact-molecules is attended with a potential energy of their mutual repulsion, equal to the actual energy expended by the same repulsion in bringing the bodies to a common velocity. Now, if the bodies be supposed to be perfectly elastic, this potential energy will be expended in separating the bodies, by altering their velocities —the loss and gain effected being the same as has been experienced during the previous interval. The previous compression is now followed by an equal recoil, and the repulsions in operation between the contact molecules, and between all those which have been urged into closer proximity, pass through the same values in the inverse order, and in an equal interval of time. No heat, or other form of energy, can then be evolved.

But if the bodies be imperfectly elastic the approximation of the molecules, and also of their atoms, during the interval of condensation, will be attended with an increase in the intensities of the attractive forces exerted by the same, as compared with those of their repulsive forces. The tendency of this change is to establish a series of new positions of internal equilibrium, in which the molecules, and their atoms, are in closer proximity than before. One result of this tendency will be that the amount of the condensation, and the duration of the process, will be prolonged. But when the end of this interval is reached the two bodies will have a common velocity, and this result will have been reached by the expenditure of a certain amount of repulsive energy between the molecules at the point of contact, just as in the case of perfectly elastic bodies. But the potential repulsive energy between these molecules, now subsisting, is not, as in the former case, expended in altering the velocities and momenta of the two bodies, but *is chiefly employed in effecting some mechanical change in the internal condition of the individual atoms, conceived to be made up of distinct parts, the recoil to which is the immediate origin of the heat developed;* and these atomic changes are attended with variations in the intensities of the mutual actions of the atoms. It is to be observed that the contiguous surfaces of the colliding bodies now separate, after they have been brought to a common velocity, chiefly by a continuation of the condensation of each body on the line of impact; brought about mainly by the variations above specified as occurring in the atomic forces. A small portion of the potential repulsive energy subsisting at the point of contact, is consumed in effecting this condensation, in opposition to a decreasing resistance; but the greater portion is employed in effecting the atomic change just mentioned, which in passing off produces an equivalent amount of heat-energy. This represents the amount of energy which is lost in the impact.

Upon the conception of an atom, and of the molecular forces, adopted in my papers on Molecular Physics, published in former Nos. of this Journal, and in view of the special theory of the essential physical characteristics of imperfect elasticity presented,* the whole matter becomes intelligible. We see that the portion of the energy of the moving bodies which is lost in the impact, is expended in disturbing the dynamic equilibrium of the electric envelopes which surround the atoms, and are immersed in their ethereal atmospheres.† The reason of this result lies in the theoretical position taken, and sustained by various considerations,* that in the case of inelastic bodies the approximation of the atoms is attended with an increase in the attractive impulses which their contiguous envelopes exert upon each other, and therefore, with an expansion of the envelopes, and consequently a diminution in the distance between the atoms that answers to the zero of effective action. It thus happens that, as the condensation goes on, a series of instantaneous positions of equilibrium of the atoms and molecules (so far as the natural molecular actions are concerned) are passed through, by reason of the change just noticed, induced in the condition of the atoms. The potential repulsive energy subsisting at the point of contact, is thus mainly expended in disturbing the equilibrium of the atomic envelopes. *The heat-energy evolved consists in the return of these envelopes to their former undisturbed condition, with attendant vibratory movements of their different layers, and the ethereal waves resulting therefrom.* As the atomic envelopes return to their original condition, with the evolution of heat, the condensation or set, subsisting at the instant of the complete separation of the bodies, will pass off, in consequence, more or less. Any permanent set that may remain after the bodies have recovered their original temperature, being chiefly due to changes in the atomic forces, resulting from alterations of the physical condition of the atoms incidental to the act of compression, will not have been attended with the expenditure of any considerable portion of the energy, or living force, that is lost in the impact.

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* This Journal, June, 1872, pp. 443 and 444.

† This Journal, May, 1872, p. 336.

The theory may be succinctly stated as follows: The destruction of the motion of approximation of the two bodies, during the first stage of the impact, will introduce into the system an amount of repulsive energy equal to that expended in bringing the two bodies to a common velocity. In elastic impact this is consumed in imparting the same loss and gain of velocity already experienced; but in inelastic impact is expended in displacing the atomic envelopes from their positions of equilibrium, and their subsequent return to their former condition is the immediate origin of the heat given off. The essential ground of the distinction lies in the fact that, with elastic bodies, the atomic forces, as exerted at a given distance, do not vary in intensity; while the reverse is true of inelastic bodies. The physical reason for it is that, in the former case, the mutual attractive actions of contiguous atomic envelopes is not altered by reason of the condensation which the bodies undergo, while in the latter it is augmented.†

4. Another no less decisive objection to the doctrine of

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* This Journal, June, 1872, p. 444.

† This Journal, June, 1872, p. 443 and 444.

atomic heat-vibrations, may be derived from the consideration that *the rapid subsidence of the supposed vibrations, both of heat and light, when their inciting cause is no longer in operation, implies a degree of resistance to the rapidly moving atoms, from the ether to which the living force of the vibrations is communicated, far greater than can be admitted to exist.* A short calculation will serve to make this evident. According to the experiments of MM. F. Lucas and A. Cazin,* the duration of a spark from an ordinary electrical machine does not exceed 4 millionths of a second. In this minute interval of time, then, a large fraction of the living force of atomic light-vibrations, incited by the electric discharge, is communicated to the surrounding ether. Now let us allow that the extent of the vibrations is as much as $\frac{1}{1000}$ of an inch (which is undoubtedly much above its actual value). The average number of undulations per second, in the waves of the different colors that make up white light, is about 550,000,000,000,000. In one second, then, each of the vibrating atoms will have traversed, in successive vibrations, the space of $0{\cdot}001 \times 550{,}000{,}000{,}000{,}000$ in. $= 8{,}680{,}555$ miles. This exceeds the velocity of the earth in its orbit (18·18 miles per second) in the ratio of 477,478 to 1. Now we will suppose that in the interval of 4 millionths of a second the average velocity of the atomic vibrations is only reduced by a small fraction, say $\frac{1}{10}$, instead of being entirely taken up, and that the retarding force of the ether remains sensibly constant while this small reduction takes place. Denote this retarding force, for a single atom, by p; the initial average velocity of vibration, above given, by v; and the interval of time in which a constant retarding force, of the intensity p, would be capable of bringing the atom to rest by t: then $v=pt$. Also let $v'=v-\frac{v}{10}$, and t' the interval of time in which the same retarding force would destroy this velocity; then $v'=pt'$. Hence $v-v'=p(t-t')$, and $t-t'=\frac{v-v'}{p}=\frac{\frac{v}{10}}{p}=\frac{v}{10p}$. This is then the expression for the interval of time in which the velocity v should be reduced $\frac{1}{10}$. The retarding force of the ether, at the velocity of 8,680,555 miles, of the atom, should exceed (if we admit the law of proportionality to the squares of the velocities) that which the same atom would experience, if moving with the velocity of the earth in its orbit, in the ratio of $(477{,}500)^2$ to 1. Let us denote this latter retarding force by p'; and suppose $T-T'$ to be the interval of time in which this force should reduce the velocity (V), equal to that of the earth in its orbit, by $\frac{1}{10}$. Then, as before,

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* Philosophical Magazine, Oct., 1872, p. 319.

$$\mathrm{V}=p'\mathrm{T},\ \mathrm{V}'=p'\mathrm{T}',\ \text{and}\ \mathrm{T}-\mathrm{T}'=\frac{\mathrm{V}-\mathrm{V}'}{p'}=\frac{\mathrm{V}}{10p'}.$$

We have, therefore, $\frac{\mathrm{T}-\mathrm{T}'}{t-t'}=\frac{\mathrm{V}}{10p'}\div\frac{v}{10p}=\frac{\mathrm{V}}{v}\cdot\frac{p}{p'}$;

$$\text{Or,}\quad \mathrm{T}-\mathrm{T}'=(t-t')\frac{\mathrm{V}}{v}\cdot\frac{p}{p'}=(t-t')\frac{1}{477{,}500}(477{,}500)^2.$$

Thus, $\mathrm{T}-\mathrm{T}'=(t-t')477{,}500=477{,}500\times 0^s{\cdot}000004=1^s{\cdot}91$.

Accordingly, if the velocity of vibration of the atoms, produced by the electric spark, is reduced by $\frac{1}{10}$ in 4 millionths of a second, the same atoms moving with the velocity of the earth in its orbit, should lose $\frac{1}{10}$ of their velocity in two seconds.

It does not seem possible that any allowable change in the assumptions upon which the foregoing calculation proceeds, can be made that will not still reveal an enormous discrepancy between the ethereal resistance computed for the orbital velocity of the earth and any actual resistance that can be admitted. It is true this discrepancy may be much lessened, for the general mass of the earth, if we allow that the resistance of the ether takes effect upon the molecules of this mass, instead of their constituent atoms; and still more if the flow of the ether through the interstices of the mass be conceived to be wholly obstructed. But we can neither take the ground that the molecules of the earth's atmosphere contain a great number of atoms, nor that it intercepts the impulses that take effect upon the ether which pervades its interstices, by reason of the earth's motion in its orbit. Much less can such a position be taken with regard to the cometary bodies, whose substance seems at times to reach the verge of possible tenuity, and yet they traverse the sea of ether often with velocities much greater than the earth's. It is a question not yet definitely settled, whether any of these bodies are sensibly resisted in their motions by the ether of space.

It may perhaps be objected to the above calculation, that the basis of it is the exceedingly rapid subsidence of the light emitted by the electric spark, and not of heat-rays. But the origin of light is conceived to be essentially the same as that of heat—if it is atomic vibrations for the one, it is also atomic vibrations for the other, differing only, to a certain extent, in rate and amplitude. If in ordinary cases the subsidence of heat is less rapid, it must be ascribed to the fact that the loss by radiation at the surface of the heated body is supplied in a good degree by heat received by conduction from the interior of the mass. In the case of a thin gas-jet, a sudden interruption of the supply is no doubt attended with an almost instantaneous large reduction of the heat evolved.

II. *Can heat consist in a motion of revolution of the atoms of bodies?* The same objections that have been urged against the theory of atomic heat and light vibrations, will hold against the present hypothesis. The hypothesis seems, in fact, to be almost a mechanical impossibility, consistently with the ordinary permanency in the properties of substances. Whether we regard the atoms as arranged in duplex or complex systems, these systems must be within the range of powerful reciprocal action, and hence must be exposed to mutual perturbations that should apparently be destructive of all permanency in their state, and so in the mechanical and physical properties of the substance to which they belong.

III. *Can heat consist in a rotation of atoms about axes?* The same objections still hold against this hypothesis as against that of atomic vibrations. To these it may be added, that upon this idea the expansive action of heat must result from ethereal vortices originating in the motion of rotation, but if such vortices have an outward or repulsive action, in a direction perpendicular to the axis of rotation, the tendency should be the reverse of this in the direction of the axis; and hence atoms that have absorbed an additional amount of radiant heat (i. e., have taken on, under the impulses of the heat waves, a more rapid rotation) should exert an expansive action in certain directions, but a contractile action in directions at right angles to these.

Other objections, of great force, might be urged against the doctrine that heat is some mode of motion of the atoms of gross matter, drawn from both physical and chemical considerations; but those which have been presented will suffice. Unless they can be effectively answered this doctrine must be unhesitatingly abandoned.

Conclusions.—The results of the foregoing discussion seem to bring us irresistibly to the conclusion that the atoms of bodies must be made up of distinct parts bound together by certain forces; and that heat must consist in some movement of relative displacement among these constituent parts of the atoms. If now we consider that every atom is capable of exerting upon surrounding atoms an effective repulsion at the more minute distances, and an effective attraction at certain greater distances, we are led to infer that the "atom," so-called, consists of a central attractive nucleus, surrounded by an envelope, or atmosphere, composed of repulsive elements. We also readily discern the possibility that heat and light may consist in some mode of motion of this outer envelope, either in its elementary parts, or as a whole. Now we have independent evidence, afforded by the entire series of electric and magnetic phenomena, that there exists a subtile form of matter, made up of mutually repulsive elements, intimately associated with the atoms of bodies, which has received the name of the electric fluid, or electric ether. It is true that it has been vaguely conectured that electricity may consist in some mysterious mode of motion of the atoms of ordinary matter, but such a mere conjecture, unsubstantiated by any decisive evidence in its favor, cannot throw an air of improbability over an hypothesis that is sustained by a multitude of actual phenomena.

It is, however, conceivable that the electric may be identical with the luminiferous ether which permeates all bodies, and is known to be physically linked to its atoms. We may then form two possible conceptions of an atom, with its essential accompaniments, viz: (1) that it consists of a true atom surrounded solely by an atmosphere of luminiferous ether; (2) that t has, in addition, an envelope of distinct electric ether immersed n the ethereal atmosphere. In view of the results of this discussion, we recognize the high probability that heat and light originate in some mode of motion occurring in the ethereal atmospheres, or in the electric envelopes of the atoms; or, more properly, in the force or forces by which such a movement is produced.

Against this conception of the origin and nature of heat, the objections that have been brought forward against the prevailng notion do not hold good. The source of heat is now transferred from the atom proper to its ethereal atmosphere, or electric envelope, one or both—and, therefore, to a form of mater nearly if not quite as subtile as the medium of light, and whose elastic forces are nearly or quite as intense. Hence the enormous rapidity of the recurring movements, and the amazng intensity of the accelerating forces in play in the evolution of heat and light, are in full accordance with the present hypothesis. Again, the change in the physical state of the "atom" what is ordinarily represented by this term) and consequently n its operative forces, which we have recognized as attendant upon the evolution of heat by impact, are effects that may reasonably be expected from a disturbance of the atomic envelopes by the force of impact.* Also, upon this hypothesis the impact of the ether of space upon the earth, and other cosmical bodies, should take effect upon the ethereal atmospheres of the atoms of these bodies, and develop electric or ethereal currents, that would eventually pass off in the form of heat-energy, without finally checking the translatory motion of the atoms. It may seem improbable, from our ordinary point of view, that such a result should follow from ethereal impulsive actions; but we have already seen (p. 192) that the same potential repulsive energy which, in the recoil of two elastic bodies compressed by impact, imparts an additional loss and gain of velocity to the bodies, is expended, in the case of inelastic bodies, in the development of an equivalent heat-energy, without changing the velocities of the bodies as a whole. The force that is here operative is the mutual repulsion between the contact-molecules, of which we can form no other definite conception than that it consists of a perpetual stream of ethereal wave-impulses, that becomes operative, in a sensible degree, so soon as the molecules at the point of contact come within a certain degree of proximity.

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* What I conceive to be the actual nature of the change which the atom undergoes, and the true process of heat-evolution, has already been briefly intimated p. 193).

ART. XXIV.—*On the Glacial and Champlain eras in New England;* by JAMES D. DANA.

THE following brief statement of some of the views I have been led to entertain, with regard to the Glacial and Champlain eras in New England, is here presented to close up my contributions on the subject and help forward discussion. It may also serve a good turn by preventing a waste of energy in combating misunderstandings, such as occurred not long since.* Fuller illustrations with regard to most of the topics, supplementary to those in my Manual of Geology, will be found in my Memoir on the Geology of the New Haven Region, in volume ii (1870) of the Transactions of the Connecticut Academy, and in papers in the volumes of this Journal for 1871. On one point, the height of the icy plateau in which the glacier-flow over New England had its head, (treated in vol. ii of this Journal, p. 324, 1871,) I give additional observations with a modification of my former conclusion.

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* Proceedings of the Boston Soc. Nat. Hist., vol. xv, p. 48, 1872. The "strictures" of the author (read before the B. S. N. H. in 1870) "on Dana's Geology of the New Haven region" are, for the most part, not on my views but on his unfortunate misunderstandings of them. In the commencement of his remarks on the Post-tertiary, he says that the Post-pliocene of Lyell corresponds to my Terrace or Recent era—the third division of the Post-tertiary; when, in fact, as my Geology shows, it is very closely equivalent of the Glacial and Champlain eras, or the first two divisions. In another place he states that the Champlain era seems to have been, in my view, one in which the ocean extended over the most of New England beneath the glacier, and the deposits made were chiefly marine:—when I have opposed both views in my memoir as well as my Geology, and have made the era that of the most extensive freshwater formations in the world's geological history. Again, he remarks that I refer to the Terrace era the terrace deposits of the river valleys and sea-shores; when, as just stated, I make these preëminently the Champlain deposits, and include in the Terrace era only the *terracing* of the Champlain beds, and the formation of some superficial deposits. Then, having made my "Terrace or Recent" era to cover a large part of the Champlain era, he institutes for the rest—the *Recent* part in his view, which he supposes I wrongly annexed to the Terrace era—another grand division of the Post-tertiary and provides it with a name; which grand division is essentially identical in its deposits with the whole of my "Terrace or Recent" era. This "comedy of errors" relates to subdivisions which are explained at length in my Geology and adopted in the memoir.

Other examples of the misunderstandings that pervade the article might be mentioned; but these are enough.

I. The Glacial period (that of the great glacier) was an era of transportation by ice, with the deposition from the glacier of only a small part of the drift, including the boulder clay; while the early part of the Champlain period, to which I refer the time of melting, was an era eminently of deposition, and also of further transportation by moving waters and floating ice.

II. The Glacial period was very long. Supposing, as I have done, one foot a week as the average rate of movement—perhaps too large an estimate—10,000 years would be required to carry a boulder one hundred miles, the distance from the northern boundary of Massachusetts to New Haven, Ct.; and how many times one hundred miles were passed over by the ice has not yet been deciphered. The progress of the melting of the glacier was for the greater part exceedingly slow; but at the close, or for the lower 500 feet, relatively rapid.

III. The general course of the movement over New England was to the southeastward. But in the larger valleys there was often a more or less perfect conformity to the course of the valley; showing that the movement of the lower part of the glacier was determined, in some degree, by the slopes of the surface beneath—just as thick pitch, descending a gently-inclined board that has large oblique furrows in its surface, would follow the general slope of the board, but have a part below diverted by the furrows. The direction of movement was determined by the general slope of the upper surface of the glacier; and this depended on the distribution of precipitation and temperature, and the position of the region of freest discharge, as well as the general slope of the land; but the influence of the valleys beneath was the same, whichever of these causes was predominant.

IV. Since the glacier was spread widely over the country and had no overhanging rocky walls or peaks, its stones and earth must have been gathered into its lower part where it lay in contact with the earth's surface. It brought to the New Haven region masses of trap, of all sizes, from small pebbles to boulders of 1,000 tons, and these must have been taken up for transportation from the trap ridges of the Connecticut valley, nearly all of which are under 1,000 feet in height above the sea. It also brought masses of sandstone from the lower hills or plains adjoining: and from veins in the rocks dug out pieces of native copper, which were dropped on the way; one such, found within two years past, a few miles north of

New Haven, weighing nearly a hundred pounds. These are mentioned as examples of what occurred everywhere. The whole surface of the country, from the slopes of the higher mountains to the low plains, contributed to its load, the glacier making much loose material by abrasion where it found none at hand. Moreover, part of this material was gathered up within a few miles of where it was deposited.

Having a thickness of 5,000 to 6,500 feet in northern New England and an average of 2,700 in southern, the pressure on the surface beneath was immense; 6,000 feet corresponding to at least 300,000 pounds to the square foot. Under this great pressure there was not only abrasion of the rocks beneath by the ice armed with stones in its lower surface, and also a crushing of softer kinds from mere pressure, but, besides, a breaking and crushing of the ice itself against the obstacles in its course, and also a pressing of the plastic material down among all the stones and gravel or sand; and thus it was able to envelop and take up into its mass the loose material.* Further, the ice of the ice-mass above must have been forced down into all openings and crevices in the rocks, so that the glacier, as it moved, had tremendous power in prying off and abrading, and must have made boulders and gravel—its chips—in immense quantities for transportation. Such work is more readily done by a continental glacier 2,500 to 6,000 or more feet thick, than by one of 500 or 1,000 feet confined in an Alpine valley; for the power of erosion increases almost exactly with the thickness of a glacier.

In consequence of the conditions just stated, the stones and earth which the glacier bore along were contained mostly within the lower 1,000 feet, and probably the larger part in the lower 500.

V. The larger rivers of New England may have continued to flow through the whole of the Glacial period, though, to the north, with diminished volume. The existence of such a period demanded only that through a long succession of years the melting in the warmer months should have carried off each year *only a part* of its snows, so that an annual addition was made to the accumulation in progress. A fourth, or a third, or more of the snows that fell each year may have melted to descend through the crevasses, and if so the streams would have been sufficiently well fed, independently of the contributions of springs, to have kept up their flow under the mantle of ice, and to have performed, to some extent, their ordinary work of erosion and transportation. Their loss from evaporation would have been very small, in their cold covered ways. As freedom from melting is not probable, so the existence of such streams may be reasonably assumed. (See, further, page 208.)

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* In experiments by Christie, and also by Tyndall, ice has been moulded into various shapes by pressure; and Tresca has produced, by forcing it through holes, long cylinders, the ice in the operation not losing, he states, its glassy texture or aspect. Tresca found the pressure requisite that of a column of water 4,000 feet high. But having unlimited time at command, as with the old glacier, the work could be done with less pressure.

VI. The melting of the glacier was brought on by a general amelioration of the Glacial climate,—probably consequent upon a diminution of the elevation of land over the higher latitudes, sinking this part of the continent below its present level (see beyond, p. 211). It went on over the general surface of the glacier, and not merely at its southern edge, though of course diminishing in rate to the northward. When the average thickness of the ice over a region was reduced to 1,000 feet, the stones and earth that had been in the ice above were now in this remaining portion; and so for 500 feet, etc.; and consequently, toward the close of the melting, very large quantities of drift must have been set free together. Sooner or later, the waters from the melting ice, descending through the crevasses or other openings, would have made streams in all the valleys, even those now dry, and lakes wherever there was no outlet, besides enlarging greatly the rivers and lakes that may have held their places through the Glacial era. There was thus, over the continent, water as well as dry land to receive the gravel and sand thrown down, and water also along the sea-border. The melting would have gone forward with increasing haste as the thickness of the ice lessened; and all the streams would thereby have become flooded far beyond modern experience, and consequently the work of transportation and deposition would have been vastly accelerated.

VII. The conditions attending the melting of the glacier being such as have been just specified, the earth and stones that fell over the dry parts of the surface would have been *unstratified;* while the part which fell into moving waters should have varied in the degree of stratification received according to the depth of the waters, and their rate of flow, and also according to the kind of material thrown in and the rate of its supply. If the earth fell in too great quantities for the waters to work over and arrange, there would have been produced only imperfect stratification, or none at all beyond a levelling off at top; while, with a more gradual supply, or a fuller flow of waters, the stratification would have been regular and general. Very rapid currents would have made layers in the formation that were obliquely laminated, the hurrying waters pushing up the sands before them as deposition went on; and where the supply of earthy material from the unlading glacier was very large and the waters abundant, the obliquely laminated layers thus made ought to have been a foot or more in thickness; for each such layer is necessarily the result of a single onward rush of the waters. Where plunging waves accompanied the rapid flow, the resulting layers would have been composed of wave-like parts, each independently laminated.* In quiet waters, the deposits should have been of all degrees of fineness and regularity down to those of clay.

The older terraced alluvium or stratified drift of the valleys of New England presents in its various parts all the different kinds of deposits here described. The material is generally stratified. Much of the alluvium over the interior has at intervals beds that are obliquely laminated; but this characteristic is most common toward the coast. The terraces of an estuary, like that of New Haven, are only the terminations of those of the river valleys which open into the region about the estuary; and the latter are identical in character with those over all New England, and part of one and the same system.

VIII. The depositions along the valleys and estuaries continued to increase in extent, long after the melting of the glacier was ended, through contributions from the unstratified drift which lay loose in immense quantities over the hills; and afterward, during the rest of the Champlain era, it went forward more slowly, from the ordinary operation of fluviatile, lacustrine and marine waters.

IX. The facts afford the following argument in favor of some of the views above stated.

(1) The prevalent stratification of the old terraced alluvium over New England is evidence of its *sedimentary* origin. (2) From the vast width of many of these alluvial regions, we infer an extraordinary flow of waters over the country. (3) The great thickness of the deposits, rising in some places, for long distances, to two hundred feet or more above the river, and no doubt originally filling the valley to the level of the upper terrace; and still more, the frequent occurrence of thick obliquely-laminated layers—one such in the New Haven region, reaching the extraordinary thickness of eight feet,—are indications of a very rapid and abundant supply of sand and gravel; and the beds of coarse stones, often intermingled, tell of currents of immense power, or of sudden falls from the floating or overhanging ice. (4) As the vast flow of waters and the vast flow of sand and gravel were concurrent events, and since the era of deposition immediately followed that of the great glacier, it seems to be a most natural inference that the final melting of the glacier set free both the water and the stones and earth.

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* Several of these points are illustrated in my Memoir on the Geology of the New Haven region. Layers of this composite kind characterize much of the "Orange Sand" in northern Mississippi, as represented by Prof. Hilgard in his Geological Report on that State, who has shown that this formation is in all probability only stratified drift.

(5) The extent of the floods throughout New England, as proved by the extent and character of the deposits, also indicates that the melting of the glacier did not occur only at the southern margin, so creeping slowly northward, but simultaneously over its wide extended surface; and if so, the melting of the glacier must have been due to a general decided amelioration of the continental climate, as above stated.

X. About the head of New Haven bay the terrace-plain has a height of from 40 to 50 feet above the sea. It is every where stratified, and the beds are generally obliquely laminated. The laminæ of the obliquely laminated layers dip southward, that is, rise to the northward, and thus show that they were deposited under the action of currents from the south, those of the incoming tidal waves. But at the entrance of the river valleys or where the river valleys merge into the region of the New Haven plain, at a level about 20 feet from the top of the deposits, (and 20 to 25 above the present sea level,) there is an abrupt change in the direction of the oblique lamination, the layers above this level rising *to the south* instead of to the north. Here is proof that a river flood had then set in that controlled the depositions in spite of the force of the incoming tidal currents. That the waters of this flood came loaded with sand and gravel in enormous quantities is indicated by the thickness of the obliquely laminated layers; that there were plunging waves in the estuary connected with both the incoming tide and the flood, is made manifest by the composite character of these layers.

XI. In the Glacial era, the land over the higher latitudes probably stood *above* its present level.

The occurrence of fiords, both in the northern and southern hemispheres, in the Glacial latitudes, is favorable to this view, as I have elsewhere stated. For they show at least that during their formation the land in these latitudes was elevated above the present water level, when more to the south it was not so; and, therefore, that the cause of the change of level was not one affecting alike the whole globe. But these fiords may have been begun long before the Glacial era, in earlier periods of elevation. No Cretaceous or Tertiary deposits occur along the American coast north of Cape Cod, while they are present to the south and thicken southward. This fact is evidence that in those eras the continent to the north was higher than now, and the part to the south lower. It also suggests that these fiords may then have been forming. It is possible that they were then finished; but the condition of level in the later Tertiary being as above stated, when land and mountains were in progress of elevation over the globe and the cooling of the earth's climates was consequently going on, it is no forced supposition that it continued so to be through the following era in which the cooling reached its maximum; and probable also that there was some increase of difference between the level of the north and south corresponding somewhat with the increase of cold.

XII. The scratches of eastern Canada, of the high land of northern New England, and of eastern and western New York and northwestern Canada, point to a part of the Canada watershed between the St. Lawrence and Hudson bay as the head of the glacier that moved southeastwardly over New England.[*] The large valley of the river St. Lawrence, over 300 miles wide between the watersheds on either side, and trending east of northeastward, afforded no discharge for the ice; and this is proof that the summit-surface of the glacier about the mouth of this river, or over the St. Lawrence bay, was somewhat higher than over the watershed to the west.

In order that the glacier ice should have flowed over the whole line of the barrier or watershed bounding the St. Lawrence valley on the south, the level of the ice over the Canada watershed must have been the higher; and so also that in the St. Lawrence valley, for the first result of the movement would have been to raise the level of the valley ice to that of the barrier in front, the law of flow being, according to the generally accepted view, much like that of a stiffly viscous fluid. But that the glacier should have abraded the White Mountain slopes 5200 feet above the sea level, and carried boulders up the north slope of Mt. Washington to 5800 feet, the ice should have stood in that region about 6000 feet above the sea. The swell of the surface from which the White Mountains rise has a height, according to Guyot, of about 1000 feet above the sea; consequently the thickness of the glacier in that part must have been at least 5000 feet; and the facts observed do not need for explanation greater thickness than this. To the northwest, north, and north-northeast of the White Mountains, over the plateau about the headwaters of the Connecticut, the general level is about 1500 feet above the sea, which would make the upper surface of the glacier, in that region, if it were of the same thickness, about 6500 feet.

But we have the means of arriving at a more certain conclusion with regard to the last mentioned altitude. The slope of the upper surface of the glacier from the northern borders of New England southeastward in the line of flow would have been, according to the laws of fluids, very nearly uniform; and its average amount would have depended on the distance of the terminal margin in the ocean. The distance from the White Mountains southeastward to the coast of Maine, near Portland, and south of it, is about 80 miles. Supposing the glacier to have extended out 50 miles to sea, making the whole length 130 miles, and to have faced the ocean with a cliff 500 feet high, the rate of slope from the level at the White Mts., 6000 feet, would have been 42 feet a mile (or 1 foot in 126). If it extended on 150 miles to sea, to the range of shallow water between Cape Cod and the banks lying just east of Nova Scotia, as is more probable, the inclination at the surface would have been 24 feet a mile (or 1 foot in 220). Taking the latter as the slope, and extending it back from the White Mountans to the northern border of New England, or the southern margin of the great St. Lawrence basin, a distance of about 80 miles, we find for the level over this border region about 8000 feet, or 2000 feet above that of the White Mountains.

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[*] See this Journal, III, ii, 324.

The following courses of scratches in Canada just north of the mountains of northern New England, are cited from Logan's Report on the Geology of Canada for 1863. In Sutton, S. 36° E.; Orford, S. 43° E.; Sherbrooke, S. 43° E.; in lat. 47° 44′, long. 69°–69° 12′, S. 49°–64° E.; near Lake Temiscouata, lat. 47° 35′ to 39′, and long. 68° 39′ to 49′, S. 48°–54° E., with one observation of S. 27° E.; on Madawaska river, lat. 47° 22′, long. 68° 19′, S. 60° E.; at Trois Pistoles, near the St. Lawrence, lat. 48° 7′, long. 69° 8′, S. 32° E. These courses are cited here, not to prove the convergence alluded to, but to show that the system of movement was the same north of the high northern New England border as south of it.

The region between this northern border of New England and the Canada watershed was literally a great basin with the southern margin the higher. The actual height of the ice over this watershed required to cause a flow across it into New England, and to produce at the same time abrasion at bottom, we have no means of calculating. The slope of the nearly even upper surface across could hardly have been less on an average than 10 feet a mile; and, as the distance is 500 miles, this slope would give for the height of the surface of the glacier over the watershed 13,000 feet. This slope over the St. Lawrence valley would have gradually fallen off into that of the New England part of the glacier.

The average height of the watershed is about 1,500 feet; and this gives 11,500 feet for the thickness of the ice on its summit. But the mean height of the mass is certainly 500 feet less, and hence the average thickness of the ice to the north was not less than 12,000 feet; while over the plateau on the northern borders of New England it was about 6,500 feet; in the region of the White Mountains, 5,000 feet; along the seashore south of Portland, 4,100 feet, the whole height there being of ice; at the terminal cliffs, 500 feet above the sea level, with the under surface of the glacier resting on the sea bottom.

It should be here stated that the accumulation of ice to the height mentioned on the Canada watershed supposes that there was no movement northward toward or over Hudson Bay. If there were such a movement, the region of greatest height might have been to the southward of the watershed (the freest discharge being still to the south) over the St. Lawrence valley. But as already mentioned, the scratches over the region from western New York and Lake Huron to eastern Canada and Maine point toward the watershed as the head of the flow; and hence, there was apparently no discharge northward into Hudson Bay, and moreover the ice must have stood high above this region of depression. It is possible that the greatest height of the glacier was a little to the north of the summit of the watershed; but more probable that the small advantage which the region of the watershed had from its elevation, and from its contrast in this respect with the Hudson Bay depression, was retained throughout.

Now while the thickness of the ice thus increased to the northward and northwestward, the amount of precipitated moisture must have decreased in that direction. The rates of precipitation for different latitudes going northward was probably nearly the same as now in our *winters*. In summer the greatest amount of precipitation in New England occurs over the higher lands of its northern half, and the amount over Canada is but a fifth less. But in winter the region of greatest precipitation is within 30 to 50 miles of the coast. Moreover, the amount decreases rapidly in going northward or northwestward, the ratio for (1) the coast region of New England, (2) northern New England, and (3) the main part of the St. Lawrence valley with the Canada watershed, being 4 : 3 : 2.* Thus the low coast slopes take the moisture in winter, not the higher mountains of the interior. This accords with the general principle, that the winds lose their moisture mostly over the first cold lands they meet. Under this principle, the main divergence in the Glacial era from existing hygrometric conditions in the winter would have been that the region of most abundant precipitation was situated a little farther south than now, and the amount of diminution—not the ratio—

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* From the excellent rain charts of C. A. Schott recently published (Tables and Results of the Precipitation in Rain and Snow in the United States, collected by the Smithsonian Institution, and discussed under the direction of Joseph Henry, Secretary; No. 222 of the Smithsonian Contributions, May, 1872), we learn that in winter, over the sea-border region of New England, 30 to 50 miles wide, the amount of rain and snow is 10 inches or more, to 12 inches, two large areas of 12 inches occurring on the southern border and others along the northeastern. Over the northern border of New England the amount is with a small exception (a narrow loop running up to Montreal) less than 8. The chart contains no line north of that of the 8-inch line, and hence does not enable us to deduce the true mean for the region. Taking it at 7½ inches, and that of the sea-border region at 10, the ratio is 3 to 4; the latter would have to be 11¼ instead of 10, to give the ratio 2 to 3.

Blodget's rain charts give 5 inches as the average for a large part of the St. Lawrence valley. The amount for the Canada watershed would be the same, or less, since, as New England shows, there is no increase northward even if there is increase of elevation. The ratio used above supposes 5 inches to be the mean for the whole.

northward would have been greater, even if the surface increased n elevation in going north in New England. We shall not herefore be led into any great error, if we take the ratio of precipitation during our winters as a basis for deductions respecting the Glacial era.

We have made the thickness of the ice in the region of the watershed 12,000 feet, and that over the northern border of New England 6,500 feet. But, following the rate of winter precipiation, 2 to 3, the latter should have been not 6,500 but 18.000 feet. The loss here indicated must have depended partly on an increased rate of flow over New England. In changing the surface slope from 10 to 24 feet a mile, the rate of flow would have been at least doubled; and this alone would have reduced the 18,000 feet to 9,000. Again, the more southern latitudes would have had a greater amount of evaporation and melting, and it is possible that thus the rest of the excess—2500 feet—was removed. If the loss from the last-mentioned source were in the Canada region *one-eighth* and in northern New England a little over a *fourth*, the above difference would have resulted. Thus the contrast in the elevation of the glacier surface over the two regions may have existed without supposing the land of the watershed above its present level.

The glacier probably extended in a southward direction at least 60 miles south of Long Island, where the depth of water s not over 250 feet; and perhaps 30 miles beyond where the depth is 600 feet, and then falls off abruptly.* The height of the upper surface of the glacier along the Connecticut valley may be thus made the subject of calculations. The line of 6,000 feet elevation (which should have run at right angles to the direction of flow, except so far as temperature was a cause of divergence) passing Mount Washington, probably crossed the Connecticut in the region of Lyme or Hanover, N. H. The distance from this region to the limit 90 miles south of Long Island is about 310 miles; whence the mean slope of the upper surface of the glacier down the Connecticut valley should have been about 19 feet a mile.

With this grade (supposing it a straight grade, which it would not have been throughout, as the flow of the general mass was southeastward) we should have for the height in the region of Northampton and Mount Tom, 4,100 feet above the sea; of Springfield, 3,800; of Hartford, 3,400; of Meriden, 3,000; of New Haven, 2,750; of central Long Island, 2,100 feet. The height of the terminal cliff 90 miles south of Long Island is here supposed to have been 200 feet.

The height of the upper surface of the ice over the central part of Connecticut probably averaged 3,250 feet, which would give about 2,700 feet for the mean thickness of the ice. But the rate of precipitation over the northern border of New England being to that of Connecticut as 3 to 4, the thickness of the ice over the latter, considering this condition alone, should have been 8,700 feet instead of 2,700: there was thence a loss of more than two-thirds of all the snow, which loss we can attribute only to melting and evaporation. If the waste from this cause over the northern border of New England was *one-fourth* of the whole precipitation, that in southern Connecticut would have been over *three-fourths;* just three-fourths, if the ice-cliff were assumed to be 500 instead of 200 feet.

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* See Author's Manual of Geology, page 441.

Whatever doubt exists with regard to the height attributed to the glacier about Mt. Washington also attaches more or less to the preceding calculations. But, to sustain our conclusion, we have now, in addition to the facts there observed, evidence that all the requisites of the Glacial era for the region from the Atlantic shoals to the Canadian watershed are satisfied by it. This is reason for believing that the error connected with the deduced height of 6,000 feet cannot be large. Moreover the Gulf Stream washes the margins of the banks in which the glacier has been supposed to have terminated, and would have determined a limit in height as well as length.

The evidence of a large amount of melting in southern New England proves that there was also much in northern, and therefore that the increase in the rate of flow of the glacier, after passing from Canada into New England, could not have been great enough to account for all the loss of thickness in the ice. If the ice lost was more than could be reasonably attributed to the temperature and rate of flow, as may have been if the slope from the Canada watershed was greater than our estimate, we should then have proof that the height of the surface of ice over the watershed was partly due to an elevation of the land.

The greater amount of precipitation over the vicinity of St. Lawrence Bay than over the watershed to the west, and also the higher latitude, account for the greater height of the ice in that region referred to on page 204.

XIII. Evidence that this era of probably high elevation (the Glacial) was followed by one in which the land stood below its present level, is afforded by the height of sea-border terraces and beaches around New England and on the St. Lawrence, their height being nearly 50 feet on Long Island Sound, and 500 feet in the vicinity of Montreal, on the St. Lawrence; in the height, equally, of the upper terrace-plain along the rivers and lakes; and in the additional fact, that the old alluvium beneath this plain was a direct result, as above stated, of the deposition by the rivers of material afforded by the melting glacier. The greater height of the river and lake terraces as we go north, and also of sea beaches, indicates that the depression increased to the northward. The special facts on this point need not be here repeated.

XIV. The sinking of the water level in the ocean over the world during the Glacial era by the loss of water to make ice cannot be estimated, because we know very little of the amount of ice. Hygrometric laws, alluded to on page 206, appear to indicate that the amount of ice did not increase to the northward over the interior of the continent. The southern line of the glacier which was near the parallel of 39° along the Ohio region must have made a very long bend northward between the meridians of 98° W. and 108° W. The marks of the incoming tide in deposits of the New Haven region, above mentioned, and in the overlying beds of the river floods that supervened, appear to show, in connection with the present height of the deposits, that during the closing part of the melting of the glacier, in the early part of the Champlain era, the water-level along the coast was not far from forty-five feet *above* the present line, *instead of below it.*

XV. There are no true *lateral* moraines of the Glacial era in New England. For the glacier was not a valley glacier, but one of continental character, although far from covering the whole continent. The ice moving over a rocky hill or ledge, where were detached blocks, and where others were becoming loosened with the passing centuries, would have gathered up the material from time to time, and afterward have dropped it at intervals, or at the final melting; and so would have made long trains of boulders, ranging over hill and valley, according to the direction of movement of the enclosing ice;—sometimes narrow lines, like the famous trains in Berkshire, first made known by Dr. Stephen Reed; generally broader, less-defined bands. The unstratified drift is actually made up mainly of such trains. But, commonly, the different trains are blended together, and are traceable only with difficulty; while, at times, they make a straight line to the ledge from which they were derived. These trains are properly moraines, but they are *under-glacier moraines*, not lateral. If lateral moraines were made during the early stages of progress of the great glacier, they must have been obliterated by its later general movement and abrasion.

The valleys of New England have throughout a high terrace along their sides; but the material is very generally stratified, and therefore is the result of deposition from waters that once flooded the valleys. They are, in fact, as has been explained, the upper part of the Champlain formation. The decline of the glacier in the opening part of the Champlain era, must have offered in some places the necessary conditions for moraines; and such have been announced as occurring in the White Mountains by Dr. A. S. Packard and also by Prof. Agassiz. But for some cause—probably, (1) the melting taking place over the surface at large, instead of along the southern edge chiefly, and (2) the diminished slope of the surface (due to the fact that the subsidence of the crust which introduced the Champlain era increased in amount to the northward), the two causes diminishing, if not stopping, over large regions, the movement of the ice—none were left over the general surface, or even along the larger valleys.

XVI. No distinct *terminal* moraines of the Glacial era have been observed in New England. The great glacier terminated in the ocean on the east, southeast and south ; and it may be added that during its decline, in the Champlain era, the melting appears to have gone forward over too vast an extent of surface for the formation of proper terminal moraines. Having a thickness on the border of 2,600 feet, it must have filled Long Island Sound as a consequence of its weight, even if the Sound—but 150 to 250 feet deep and 15 to 20 miles wide,—had not been partly obliterated by a change in the water-level; and thence it stretched on over the island (which is about 15 miles wide) to the ocean. If the land of Long Island were but 120 feet higher above the water-level than now, the southern coast would have been 20 miles outside of the present line; and, whether so or not, the glacier, if 2,100 feet thick over the island, probably lay on the sea bottom for ninety miles out, as already remarked. The shallowness of these waters is probably due partly to deposits from the glacier; and also that of the waters between Cape Cod and Nova Scotia, where the part of the glacier over Maine is supposed to have terminated.

The universal covering of drift over Long Island, holding great numbers of boulders from New England and New York, many of enormous size, suggests the idea of a terminal moraine deposit. But whether the drift was dropped mostly from the under side of the glacier as it melted, or from its melting southern edge, is open to question. The latter seems most probable.* In either case the deposition of the drift on the island was probably the work, not of the Glacial era, but of the early Champlain era or that of the melting:—certainly so, if the glacier over the island in the Glacial era had the thickness above supposed.

As already stated, whatever there may be of local moraines in the White Mountains belongs to the period of melting, and therefore to the opening part of the Champlain era.

XVII. The Champlain† era, as the term has been used by me.

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* The writer's investigations have not extended to Long Island. Mather in his N. Y. Geol. Report (4to, 1843) has an excellent chapter on the drift of Long Island, and the distribution of its boulders.

† The term *Champlain* was first applied to deposits of this era on Lake Champlain by Hitchcock in the Vermont Geological Report. "Champlain Division," had been previously used in the Final Reports of the Geological Survey of New York,

includes all the time from near the beginning of the melting of the glacier, down to that in which these old alluvial or Champlain deposits became terraced in consequence of a general rising of the land, when what I have called the Terrace or Recent epoch began. According to my view, the terraces are *not* a result of depositions *in* the Terrace era, (as is represented in the Vermont Geological Report (4to, 1861)); they are due simply to the wearing away of portions of the old Champlain alluvium (which once filled the valley across, excepting a river channel) by the river, while the elevation of the land above referred to was in progress. The depositions of the Terrace era along rivers are merely superficial over the Terrace-plains, the thickest being those of the lower flats or the great flood-ground of the streams.

XVIII. If the above views are correct, then, as I have before announced, there was, in the Post-tertiary or Quaternary age, over the higher latitudes of North America, or large portions of them, an upward movement for the Glacial era, a downward below the present level, for the Champlain era, and an upward again for the Terrace era, with the change in each case greatest to the north—that is, to a limit north yet undetermined. But such a general system of changes does not preclude the occurrence of minor oscillations up or down, in different regions during each of these eras, or imply that such changes may not be now in progress.

NOTE TO PAGE 204.—The fact that the glacier in the St. Lawrence valley was higher toward the Bay or above the mouth of the river, points to the conclusion that in Lower and part of Upper Canada, the scratches which have the course of the valley must have been made by a movement of the ice (of the lower part of the glacier) *up stream*, if made in the Glacial era. And none could have been made at any time by a movement *down stream*, unless, owing to an unequal rate of melting (in the opening of the Champlain era), the height of the glacier became the greatest up stream, so that the slope of the upper surface was down stream.

ART. XXIV.—*On some new species of Fossils from the Primordial or Potsdam group of Rensselaer county, N. Y.* (*Lower Potsdam*); by S. W. FORD.

Archæocyathus? Rensselaericus, sp. nov.

THE only specimen clearly belonging to this species that has come under my notice is exceedingly small, being only 0·30 of

by Mather and Emmons for the whole of the Lower Silurian of New York; but not by Vanuxem or Hall. Prof. Hall, in his works, has, like most others, employed the name *Lower Silurian* instead. As there is nothing in my view to be gained from restoring to the Lower Silurian the term Champlain, and no likelihood that it will ever be so restored, this term is here retained for a division of the Post-tertiary or Quaternary.

an inch in length, and having a diameter of not more than 0·16 of an inch at the larger extremity, when perfect. This specimen is, in appearance, a slender, delicately fluted cone, about one-third of which, including the apex, is imbedded in the rock. Of the remainder a considerable portion is in a badly damaged condition, the outer wall, with the greater part of its underlying septa, having been partially torn away. Such portion as remains uninjured, however, is in an excellent state of preservation, and shows the leading features of structure in a very perfect manner. There remains, notwithstanding, much yet to be desired in order to completely characterize the species; and I have deferred any special notice of it hitherto in the hope of being able to obtain other, and possibly more perfect, specimens; but failing in this, I have thought it advisable to carry the description as far at this time as the material at hand will permit. The species may be described for the present as follows:

1.

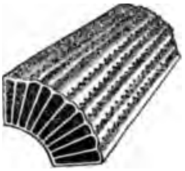

Fig. 1.—Portion of a specimen of *A.? Rensselaericus*, extending from the larger end to the first annulation, greatly enlarged.

Elongate, conical, straight, gradually expanding from the base upward. Cup moderately large, depth unknown. Outer wall thick and strong, inner wall apparently much thinner. Radiating septa thin, numerous, not far from forty, judging from the number seen, sometimes a little irregular in their spacing. Dissepiments slender, occasionally absent, at other times dividing the interseptal spaces or loculi for a short distance into several compartments. Surface faintly annulated and longitudinally marked by numerous low, rounded ridges, with shallow intervening furrows, the ridges and furrows of about equal width. The ridges mark the position of the loculi, while the middle of each furrow indicates the place of one of the septa. Along each furrow and running its entire length are two straight rows of minute, closely arranged, circular pores opening into the loculi. The rows of pores of any given furrow are separated from each other by a thin strip or plate of the outer wall, corresponding in position and thickness to the septa; and it is a singular fact that the pores of either row are arranged alternately not only with respect to the other, but also with respect to those of the succeeding row in the next nearest furrow. The pores all communicate with the interior close to the septa where these latter join the outer wall; and as the rows of pores along any given furrow lead into distinct though adjacent loculi, it follows that all of the loculi were connected with the general surface by means of a double set of apertures. Whether the inner wall and radiating septa are perforate has not yet been made out. Color of the fossil, in gray limestone, when a little weathered, light brown.

The above are the characters, so far as known, of this interesting and beautiful species, embracing only such as are displayed by the specimen mentioned at the beginning. These characters, taken together, are quite sufficient to distinguish it readily from any described species; while in respect to several of them, such as its straight form, longitudinally ribbed exterior, and remarkable poriferous system, the species appears to me at present sufficiently distinct from *Archæocyathus* to constitute a new though closely allied genus. Should a further study of it confirm this opinion, I propose to call it *Archæocyathellus.* Until, however, more can be said about it, I prefer to class it as above.

It is not improbable that the specimen upon which the description is based may be only the apical portion of a much larger and possibly curved specimen.

Occurs in conglomerate limestone of the Potsdam group at Troy; and also, doubtfully, in the condition of casts, in even-bedded limestone of the same locality. Collected by the writer.

Obolella nitida, sp. nov.

Shell transversely sub-oval, small. Dorsal valve gently but irregularly convex, the greatest elevation occurring at a point about one-fifth the length of the valve from the apex. From this point the beak curves sharply down to the hinge-line which it almost touches. The hinge-line itself is slightly curved and apparently equal to about one-third the width of the shell. At the most elevated point of the valve commences a well-defined median depression, which extends forward for a distance of about one-half the length of the valve, gradually widening and becoming more shallow till it disappears. A portion of the dorsal valve close to the margin is sometimes nearly flat all around. The internal markings are not well enough shown in any of the specimens that I have seen to admit of description. The surface is ornamented with very fine concentric striæ and numerous close-set radiating striæ, the whole just visible to the unassisted eye.

The ventral valve is not certainly known. The width of the largest dorsal valve that I have seen is 0·14 of an inch and the length 0·10 of an inch.

Occurs in both even-bedded and conglomerate limestone of the Potsdam group at Troy. Collected by the writer.

Scenella retusa, sp. nov.

Shell small, rather strongly convex, aperture ovate, sides curved. Apex obtuse, nearly central, curving down a little toward one side. On the side toward which the apex is directed there are two faint grooves commencing near the tip of the apex and diverging to the margin. On the side opposite there is a well-marked carina running from the apex to the margin along the line of the longer axis of the shell. The slope of the shell is unequal, being most rapid toward the margin to which the apex inclines. The surface is marked by a few fine concentric and radiating lines, the latter only visible under a magnifier, and with obscure imbricating lines of growth.

2.

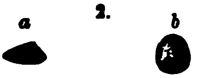

Fig. 2.—*Scenella retusa*; a, side view; b, upper view. Slightly enlarged.

Length of the largest specimen obtained, 0·16 of an inch; height about 0·08 of an inch. Occurs in both even-bedded and conglomerate limestone of the Potsdam group at Troy, associated with the preceding species. Collected by the writer.

This species is closely related to *Scenella reticulata*, the only hitherto published species of the genus, described by Mr. Billings in the Canadian Naturalist for July last from the Menevian group of Newfoundland. That species is, however, considerably larger than ours; and is, further, destitute of the diverging grooves which exist in *S. retusa*, and by which this latter species may be easily recognized.

Hyolithes Emmonsi, sp. nov.

Shell elongate, slender; apex neatly pointed, transverse section sub-triangular. Sides gently rounded and meeting to form a tolerably prominent though often scarcely perceptible dorsal ridge in the forward part of the shell, which quickly dies down, so that a transverse section taken near the apex would be almost a semi-circle. Ventral side flattened, with a wide, shallow depression along the middle, which runs the whole length of the shell; lateral edges rounded up to the sides. The most projecting point of the lateral walls occurs close to the ventral side. When the width is 0·24 of an inch the depth is 0·18 of an inch. The walls of the shell are thick and appear to be made up in some instances of successive layers or laminæ. The surface is ornamented with very fine concentric striæ, which run directly around the shell or at right angles to its longitudinal axis. The tubes sometimes attain a length of two inches, even when imperfect; but the majority of the specimens in my possession are less than an inch in length.

3.

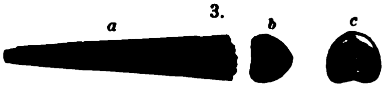

Fig. 3.—*Hyolithes Emmonsi*; a, ventral view of an imperfectly terminated specimen; b, transverse section; c, operculum, enlarged two diameters.

The operculum has the same contour as a transverse section of the shell taken at about the mid-length, and is, accordingly, distinctly emarginate at the middle of the border of the ventral limb. The ventral limb itself is in the main flat, or nearly so, and embraces not far from two-thirds of the whole superfices of the operculum. Through the middle of it, beginning at the emargination, runs a low, rounded, conical elevation having the apex directed toward the dorsal limb and slightly encroaching upon it. The dorsal limb, unlike the ventral, is highly convex, except a narrow space near the margin, which is flat. A narrow groove, extending from the apex of the cone just mentioned, or nucleus of the operculum, to the margin, occupies the central portion of this limb, and divides it into two equal parts. (A similar division is frequently well shown in the opercula of adult specimens of *Hyolithes Americanus.*) A portion of the operculum about the nucleus, of a triangular shape, is sometimes more elevated than the rest of the surface, and appears like a little plate added for strength. The surface is covered with fine, thread-like, concentric striæ.

In the slender form of the shell, the direction of the surface lines of the same, and the internal thickening already noticed, this species approaches closely the structure of a *Salterella.* Especially is this true when the specimens are quite small, as is usually the case; and in an earlier communication (this Journal for July, 1871), published prior to the discovery of specimens of its operculum, the species was referred by me to that genus. It may be readily distinguished from either of the species of *Hyolithes* found with it by the direction of the surface lines of the shell and its distinctly hollowed ventral side.

Dedicated to the late Dr. Ebenezer Emmons, the eminent author of the "Taconic System."

This species occurs in both conglomerate and even-bedded limestone of the Potsdam group at Troy, associated with *H. Americanus*, *H. impar* and *Hyolithellus micans.* It is not an abundant species, neither can it be considered as of rare occurrence. In a small specimen of the shell in my collection a transverse fracture shows a buckler of *Agnostus? lobutus* lying within the tube.

Dec. 3d, 1872.

Art. XXV.—*Discovery of a new Planet;* by Dr. C. H. F. Peters. (Letter to one of the editors dated Litchfield Observatory of Hamilton College, Clinton, N. Y., Feb. 7, 1873.)

A PLANET, supposed to be new, was found by me night before last, and has been observed as follows:

	Ham. Coll. m. t.	App. α (129)	App. δ (129)
1873, Feb. 5,	$15^h\ 21^m\ 53^s$	$9^h\ 16^m\ 32^s{\cdot}94$	$+15^\circ\ 31'\ 50''{\cdot}8$
6,	11 34 52	9 15 49 ·97	+15 38 24 ·1

hence showing a motion in 24 hours of $-51''$ in right ascension and of $+7'\,39''$ in declination. The magnitude is 9·5 (on Argelander's scale).

SCIENTIFIC INTELLIGENCE.

I. CHEMISTRY AND PHYSICS.

1. *Improved Filter-pump.*—Prof. T. E. THORPE has devised an improved form of Jogno's modification of the filter-pump, which possesses some advantages over the ordinary form. It consists of a vertical tube A about one meter in length and from 8 to 10 millimeters in diameter, having inserted perpendicularly into it, a short distance below the upper end, a side tube B, about 5 centimeters long. The upper end of A, which is cut off obliquely, is connected by means of a strong but sufficiently elastic rubber tube with the stopcock regulating the water-supply. In the original form of the apparatus the end of the side-tube was narrowed somewhat, so that when the end of the rubber tube from the flask to be evacuated was pushed into it, an air-tight joint was formed. The end of the rubber tube within B was closed with a small piece of glass rod, and a short longitudinal slit cut through the tube with a chisel, forming a Bunsen valve, which opened to allow the air to pass from the flask, but prevented its return. As the elasticity of this valve diminished after long use, impairing its efficiency, Prof. Thorpe has devised another form of valve in which the evil is remedied. A hollow metal cone is soldered into the tube B near its junction with A, the point of the cone being turned toward the outer end of B. Near its apex the cone is pierced with a number of small holes, and within it is placed a filter-shaped piece of unvulcanized sheet rubber, which is fastened by a screw to the apex of the cone. By its elasticity the india-rubber sheet presses firmly against the sides of the cone and effectually prevents the entrance of air or water from A, but readily yields to the pressure of the air from the flask. A manometer tube inserted into B beyond the valve indicates the degree of the exhaustion. When water is admitted the elastic tube pulsates, alternately closing and opening the upper end of A, and the rapidity of the pulsations is regulated by a movable arm capable of being clamped in any desired position, so that the direction of the supply-tube can be changed at will.

The action is similar to that of the hydraulic ram, and the apparatus, unlike the earlier form, requires only a sufficient head of water, and no fall beyond that in the tube A, from the lower end of which it simply flows away to the waste pipe. As the whole apparatus can be mounted on a stand about three feet in height, and can be attached to any stopcock in the service-pipe, its advantages over the ordinary filter-pump in the matter of convenience and portability are obvious.—*Phil. Mag.*, Oct. 1872, p. 249.

A. W. W.

2. *Photographic reproduction of Diffraction-gratings.*—Experiments made by Hon. J. W. STRUTT, some months since, with a view to the production of photographic copies of diffraction-gratings ruled upon glass, have given interesting and valuable results, of which he gave an account in a communication read before the

oyal Society, June 20, 1872. The account is republished in the hil. Mag. for Nov., 1872. The ruled plates were laid upon glass ates sensitized in the usual manner, and the prints were made in ie same manner as from ordinary negatives. Both wet and dry nsitive plates were used, with but little difference in the results. he photographic gratings gave brilliant spectra, and were but ttle inferior to those ruled upon glass. In the course of the exeriments, trial was made of plates covered with a film of bichroatized gelatine. The gratings thus made possessed a high degree transparency, and were found to be better than the ordinary iotographs; and although there was some uncertainty attending ieir production, the best obtained appeared to be even superior the originals on glass. They give very brilliant spectra, and ie definition of the lines is surprisingly good. They can be used ery conveniently in an ordinary spectroscope, by putting them in ie place of the prism. Gratings having 6000 lines to the inch are now iccessfully made, and as their cost is trifling compared with that the ruled ones, they will be much more accessible to experimenrs. As the thickness of the glass upon which they are mounted small, the absorption of the rays is very slight, and they offer nsiderable advantages in researches upon radiant heat, as they ay replace to a large extent the costly and inconvenient prisms rock-salt. A. W. W.

II. GEOLOGY AND NATURAL HISTORY.

1. *Additional note on the Glacial era in New England*, by J. . DANA.—The following is a point of interest, although we can eat it only with indefinite suppositions.—If the thickness of the acier over the New England border was, as has been deduced, 500 feet (the height of the upper surface being 8000 feet above ie sea level), and the precipitation equal to producing 36 inches thickness in a year, supposing no loss it would take 2,170 years r the 6,500 feet of ice to have accumulated. A loss of one-half evaporation would have doubled the length of time; of ninenths, would have made it 21,700 years. There would also have en a loss, after a while, from the flow of the glacier; but if the w was only one foot a week (the estimate on page 199), this cause ould have taken off less than a twelfth of the annual addition of inches.

Again, the thickness of 6500 feet being acquired, the precipitaon would have continued, though probably with some diminution a diminution that had been gradually introduced. Taking the aount at 24 inches, and the loss from the movement of the glacier o to two and a half inches, there would have been required a rther loss of about nine-tenths of the whole by evaporation and elting to have kept the top of the glacier at a nearly uniform level. he amount of loss from evaporation and melting was undoubtlly far greater than has been estimated on page 207; for that om evaporation is large even in the Arctic; and both sources waste may in some years have carried off all the snows that fell. But did this waste ordinarily amount on the northern border of New England to nine-tenths of the whole precipitation? Or, was the rate of motion greater than one foot a week? Or, did the glacier continue to increase in thickness beyond 6,500 feet in that region? If the average thickness over the White Mountain region can be definitely *proved*, the last of these queries will have been answered (as is obvious from page 205), and the doubts would be confined to the other two suppositions. If observations in the Arctic shall furnish something definite as to the probable loss by evaporation, this would still further diminish the doubts.

It may be that the diminution in the rate of precipitation consequent on the extension of the glacier southward (on the principle mentioned on page 206) was the chief means in fixing an ultimate limit to the thickness of the glacier.

2. *On Trains of boulders, and on the transport of boulders to a level above that of their source;* by Dr. STEPHEN REED. (Communicated.)—I have called attention frequently, in various ways, to trains of boulders extending from a hill in Canaan, Columbia Co., N. Y., across Richmond, Lenox and Lee, Mass.* Two of these trains are very distinctly marked—the north one emphatically so. They have been often visited by geologists, and several theories published. I have been aware, for several years, that a train some two miles south of these was well marked for half a mile on the east side of the hill—locally called Dean's Hill—between Canaan and Richmond; but a convenient time to trace it to its source was not found until the last season.

Leaving the cars—Boston and Albany—at "Edwards" station, and passing up the valley leading to New Lebanon Springs, I found, a little north of the Baptist chapel, a few well marked blocks. I took their back track to the hill west of the valley and soon found a very limited outcrop of the rock in question. I passed on at the foot of the hill, through an open field, perhaps eighty rods, without seeing a specimen, then found another line of blocks. I followed this into the woods and soon found another outcropping. Both of these were small in extent, situated low on the slope of the hill, and had sent but few blocks across the valley to the eastern hill. Continuing northward, a half mile of rough walking placed me in a wilderness of boulders, generally from five to twenty feet in diameter, some much larger. No outcropping rock was in sight. The boulders extended, as far as the woods would permit me to see, east and west. Following my army of venerable guides, I went west nearly to the top of the hill before any rock, *in place*, appeared. At last I stood on the bare ledge—the object of my pursuit. Although I had no doubts of success from the first, yet it was not without emotion that I followed, by the laggards they left behind, the path of these travelers across the valley and up the long ascent of the eastern hill, until they mingled with my old acquaintances on its summit.

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* An early account of these trains of boulders, as observed by Dr. Reed, is given in Hitchcock's Geological Report of Massachusetts, and the latest in Lyell's Antiquity of Man, chapter xviii.

ally not more than twenty rods wide, and is well Richmond valley, over the hill, across the Canaan e Canaan hill, to its source. This is somewhat south of the outlet of "Whiting's Pond." It of the Richmond hill nearly west of the Richation. The compass course is S. 35°–45° E.

d to find in this train a corroboration of a stateome thirty years since, that the blocks were deand more numerous on the eastern than on the ies of the hills. A section of forty and I think rods, on the east side of either the Richmond or , will give larger blocks and more in number than estern slope of Richmond hill.

s a direct bearing on the mode of transportation, er well known, viz: that the grinding and grooving the north and west sides, it points to glaciers as

oof of the same power is found in the elevation of e their source. In Canaan, Ct., near the Housatonic ridge of a peculiar rock, a mixture of lime, quartz —masses of which have been carried to the Goshen ttered widely over that town. I measured a block length and almost that in breadth, and ten or lying on some of the highest land in Goshen, Ivy cepted, and I am told there are others much larger. period of this town's history it is said that a limeilt near one of these boulders of Canaan rock, and nhabitants supplied themselves with lime for several g a huge rock which yet remains. Now these blocks dred—I think a thousand—feet above any part of the gave them birth. This is a common feature in these drift action. Water, unconfined, does not carry nuch above its own level, while glacier ice does.

n., 1873.

ery important facts respecting the ascent of boulders ls by the help of the glacier are of the same kind with It. Katahdin in Maine, where, according to different obulders of fossiliferous Devonian rocks occur over the s. Mr. J. DeLaski states (this Journ., III, iii, 27) that s occur at a height of 4385 feet above the sea (which is ousand feet of the summit) on the south side of this ountain, and 3,000 feet above the low region—fifteen to miles distant to the north—from which the fossils were

J. D. D.]

lts of the Earth's Contraction; by R. MALLET, Esq.— wing is an extract from the introductory chapter of the iced on page 140 (pp. 65 to 75).—EDS.]

cubic mile that we have thus supposed cut out of the ust at the surface were of the hardest known granite or , it would be exposed to a crushing tangential pressure [as a result of contraction from cooling] equal to between 400 and 500 times what it could withstand, and so must crush, even though only left unsupported by the nucleus beneath, to the extent of $\frac{1}{400}$ or $\frac{1}{500}$ of its entire weight. And what is true here of a mile taken at the surface, is true (neglecting some minute corrections for difference in the co-efficient of gravity, etc.) if taken at any other depth within the thick crust.*

The crust of our earth, then, as it now is, must crush, to follow down after the shrinking nucleus—if so be that the globe be still cooling, and constituted as it is; even to the limited extent to which we know anything of its nature—it must crush unequally, both regarded superficially and as to depth; generally the crushing lines being confined to the planes or places of greatest weakness; and the crushing will not be absolutely constant and uniform anywhere, or at any time, or at any of those places of weakness to which it will be principally confined, but will be more or less irregular, quasi-periodic, or paroxysmal: as is, indeed, the way in which all known material substances (more or less rigid) give way to a slow and constantly increasing, steady pressure.

We have now to ask, *How much* of this crushing is going on at present year by year? And the answer to this depends upon what amount of heat our world is losing into space year by year.

Geologists who have taken on trust the statement, that La Place has proved that the world has lost no sensible amount of heat for the last 10,000 years seem generally to suppose that to be a fact; but in reality La Place has *proved* nothing of the sort, as those geological teachers who have echoed the conclusion should have known, had they deciphered the mathematical argument upon which it has been supposed to rest.

By application of Fourier's theorem (or definition) to the observed rate of increment of heat in descending from the geothermal *couche* of invariable temperature, and the co-efficients of conductivity of the rocks of our earth's crust, as given by the long-continued observations made beneath the Observatories of Paris and of Edinburgh, it results that the annual loss of heat into space of our globe at present is equal to that which would liquefy into water, at 32° Fahr., about 777 cubic miles of ice; and this is the measuring unit for the amount of contraction of our globe now going on. The figures are not probably exact, for the data are not on a basis sufficiently full or exactly established as yet; but they are not very widely wrong, and their precise exactness is not material here. Now, how is this annual loss of heat (great or mall, as we may please to view it) from the interior of our globe lisposed of?

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* The Rev. O. Fisher, M.A., F.G.S., in a most interesting and valuable paper, "On the Elevation of Mountain Chains by Lateral Pressure, its Cause, and the Amount of it, with a Speculation on the Origin of Volcanic Action," read, April, 1868, and published in the Transactions of the Cambridge Philosophical Society, vol. xi, Part III, in 1869, has deduced the necessary crushing of the earth's crust by a different but closely analogous method. I had not seen this paper until after my own was in the hands of the Royal Society. The author's volcanic views are wholly different from my own, and do not appear to me equally valid with his notions as to elevation.—R. M.

What does it *do* in the interior? We have already seen that it s primarily disposed of by conversion into work; into the work f diminishing the earth's volume as a whole, and in so doing rushing portions of the solid surrounding shell.

But does the transformation of lost heat into the work of vertial descent, and of the crush as it follows down after the shrinking ucleus, end the cycle? No. A very large portion of the mehanical work thus produced, and resolved, as we have seen, into angential crushing pressure, is retransformed into heat again in he very act of crushing the solid material of the shell. If we see cartload of granite paving-stones shot out in the dark, we see ire and light produced by their collision; if we rub two pieces of uartz together, and crush thus their surfaces against each other, e find we heat the pieces and evolve light.

The machinery used for crushing by steam-power hard rocks nto road metal, gets so hot that the surfaces cannot be touched.

These are familiar instances of one result of what is now taking lace by the crushing of the rocky masses of our cooling and decending earth's crust, every hour beneath our feet, only upon a astly greater scale. It is in this local transformation of work nto heat that I find the true origin of volcanic heat within our lobe. But if we are to test this, so as, in the only way possible, o obtain a true solution of this great problem, we must again sk the question, *How much?* and to answer this, we must deterine *experimentally* how much heat can be developed by the rushing of a given volume, say a cubic mile, of such rocky maerials as we know must constitute the crust of our globe down to he bottom of the known sedimentary strata, and extending to uch crystalloid rocks as we may presume underlie these. We ust also obtain, at least approximately, what are the co-efficients f *total contraction* between fusion and atmospheric temperature f such melted rocks, basic and acid silicates, as may be deemed epresentative of that co-efficient for the range of volcanic fused roducts, basalts, trachytes, etc., which probably sufficiently nearly oincide with that of the whole non-metallic mass of our globe.

The first I have determined experimentally by two different ethods, but principally by the direct one of the *work* expended n crushing prisms of sixteen representative classes of rock; the pecific gravities and specific heats of which I have also determined.

If H be the height of a prism of rock crushed to powder by a ressure, P, applied to two opposite faces, which, when the prism as been reduced to its volume in powder, has acted through a nge of $H - t$, then

$$\frac{P \times (H-t)}{772}$$

the heat corresponding to the work expended in the crushing, xpressed in British units of heat. The following were the rocks xperimented upon: Caen stone, Portland (both oolites), magnesian limestone, sandstones of various sorts, carboniferous limestones (marbles), the older slates (Cambrian and Silurian), basalts, various granites and porphyries, thus ranging from the newest and least resistant to the oldest and most resistant rocks. The results have been tabulated, and are given in detail in my paper, now in possession of the Royal Society. The minimum obtained is 331 and the maximum 7,867 British units of heat developed, by transformation of the work of crushing one cubic foot of rock. If we apply the results to a thickness of solid crust of 100 miles (British), of which the upper twenty-one miles consist of Neozoic, newer Paleozoic, older Paleozoic and Azoic rocks in nearly equal proportion as to thickness, and the remaining eighty miles of crystalloid rocks (acid and basic magmas of Durocher) of physical properties which we may assume not very different from those of our known granites and porphyries—and which, in so far as they may differ, would give a still *higher* co-efficient of work transformed into heat than I have attributed to them by ranging them as only equal to the granites, etc.—then we obtain a mean co-efficient for the entire thickness of crust of 100 miles of 6,472 British units of heat, developable from each cubic foot of its material, if crushed to powder. It results from this that each cubic mile of the mean material of such a crust, when crushed to powder, develops sufficient heat to melt 0·876 cubic miles of ice into water at 32°, or to rise 7·600 cubic miles of water from 42° to 212° Fahr., or to boil off 1·124 cubic miles of water at 32° into steam of one atmosphere, or, taking the average melting point of rocky mixtures at 2,000° Fahr., to melt nearly three and a half cubic miles of such rock, if of the same specific heat.

Of the heat annually lost by our globe and dissipated into space, represented by 777 cubic miles of ice melted, as before stated, the chief part is derived from the actual hypogeal source of a hotter though not necessarily fused nucleus, and nearly, if not wholly, is quite independent of the heat of vulcanicity, which is developed as a consequence of its loss or dissipation. But were we to take the extreme case, and suppose it possible that all the heat the globe loses annually resulted from the transformation of the work of internal crushing of its shell, we shall find that the total volume of rock needed to be crushed in order to produce the required amount of lost heat is perfectly insignificant as compared with the volume of the globe itself, or that of its shell. For, as 1·270 cubic miles of crushed rock develops heat equivalent to that required to melt one cubic mile of ice to water at 32°, and if we assume the volume of our globe's *solid* crust to equal one-fourth of the total volume of the entire globe, 987 cubic miles of rock crushed annually would supply the whole of the heat dissipated in that time. But that is less than the *one sixty-five millionth* of the volume of the crust only.

But a very small portion of the total heat annually lost by our globe is sufficient to account for the whole of the volcanic energy of every sort, including thermal waters, manifested annually upon our earth. In the absence of complete data, we can only approximately calculate what is the annual amount of present volcanic energy of our planet. This energy shows itself to us in three ways: 1. The heating or fusing of the ejected solid matters at volcanic vents. 2. The evolution of steam and other heated elastic fluids by which these are carried. 3. The work of raising through a certain height all the materials ejected. To which we must add a large allowance for waste, or thermal mechanical and chemical energy ineffectually dissipated in and above the vents. All these are measurable into units ot heat.

I have applied this method of calculation to test the adequacy of the source I have assigned for volcanic heat, in two ways, viz: 1. To the phenomena presented during the last two thousand years by Vesuvius, the best known volcano in the world; and 2. To the whole of the four hundred and odd volcanic cones observed so far upon our globe, of which not more than one-half have ever been known in activity

It is impossible here to refer to the details of the method or steps of these calculations. The result, however, is, that making large allowances for presumably defective data, *less than one-fourth* of the total telluric heat annually dissipated (as already stated in amount) is sufficient to account for the annual volcanic energy at present expended by our globe.

It is thus represented by the transformation into heat of the work of crushing about 247 cubic miles of (mean) rock, a quantity so perfectly insignificant, as compared with the volume of the globe itself, as to be absolutely inappreciable in any way but by calculation; and as its mechanical result is only the vertical transposition transitorily of material within or upon our globe, the proportion of the mass of which to the whole is equally insignificant, so not likely in any way to produce changes recognizable by the astronomer.

Space here forbids my entering at all upon that branch of my investigation which is based upon the experimental results, above mentioned, of the total contraction of fused rocks: for these, the original paper can, I hope, be hereafter referred to. I am enabled, however, to prove thus how enormously more than needful has been the store of energy dissipated since our globe was wholly a melted mass, for the production, through the contraction of its volume, of all the phenomena of elevation and of vulcanicity which its surface presents. And how very small is the amount of that energy in a unit of time as now operative, when compared with the same at very remote epochs in our planet's history.

I have said that if we can find a true cause in Nature for the origination of volcanic *heat*, all the other known phenomena, at and about volcanic vents, become simple. Lavas and all other solid ejecta of volcanoes, from all parts of the earth's surface, as well as basalts, present in chemical and physical constitution close resemblance, and may be all referred to the melting of more or less fusible mixtures of siliceous crystalloid rocks with aluminous (slates, etc.) and calcareous rocks. Their general chemical composition, and the higher or lower temperatures of fusion resulting therefrom, together with the higher or lower temperatures to which they have been submitted at the different volcanic foci, determine their difference of flow (under like surface conditions) and of mineral character after ejection and cooling.

St. Clair de Ville and Fouqué have shown that the gaseous ejections, of which steam forms probably 99 per cent, are such as arise from water admitted to a *preëxistent focus of high temperature.* Whether it be sea or fresh water is not material, when we bear in mind that the chemical constituents found in sea water and in natural fresh waters that have penetrated the soil are, on the whole, alike in kind and only differ in proportions. But I must pass almost without notice all the varied and instructive phenomena which are presented by volcanic vents, for to treat of these at all would be to more than double the size of this sketch.

In the source that has been pointed out as that from which volcanic heat itself is derived, viz., the secular cooling of our globe, and the effects of that upon its solid shell, we are enabled to point to that which is the surest test of the truth of any theory—that it not only enables us to account for all the phenomena, near or remote, but to predict them. We see here linked together as parts of one grand play of forces, those of contraction by cooling, producing by *direct* mechanical action the elevation of mountain chains, and by their *indirect* action, by transformation of mechanical work into heat, the production of volcanoes; and both by direct and by indirect action of earthquakes, never previously shown to have thus the physical connection of one common cause, but merely supposed, more or less, to be connected by their distribution upon our earth's surface.

We now discern thus the physical cause *why* volcanoes are distributed, viewed largely, linearly, and follow the lines of elevation; we see equally why their action is uncertain, non-periodic, fluctuating in intensity, with longer or shorter periods of repose, shifting in position, becoming extinct here, appearing in new activity or for the first time there. We have an adequate solution of the before inexplicable fact of their propinquity, and yet want of connection. We have an adequate cause for the fusion of rock at local points without resorting to the baseless hypothesis of perennial lakes of lava, etc.

For the first time, too, we discern a true physical cause for earthquake movement, where volcanic energy does not show itself. The crushing of the world's solid shell, whether thick or thin, goes on *per saltum* and at evershifting places, however steadily the tangential pressures producing it may act.* Hence crushing *alone* may be shown to develop amply sufficient impulse to produce the most violent earthquakes, whether they be or be not at a given place or time connected with volcanic outburst or possible injection, or with tangential pressures, enough still, in some cases, to produce partial permanent elevation.

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* Yielding to the tension resulting from the earth's contraction is the chief cause of earthquakes presented by the writer in his Manual of Geology (1862), and previously as early as 1847, in articles on the Results of the Earth's Contraction, published in volumes iii and iv of the second series of this Journal.—J. D. D.

When subterraneous crushing takes place, and the circumstances of the site do not permit the access of water, there may be earthquake, but can be no volcano; where water is admitted, there may be both.

And thus we discern why there are comparatively few submarine volcanoes, the floor of the ocean being, on the whole, water-tight—"puddled," as an engineer would say, by the huge deposit of incoherent mud, etc., that covers most of it, and probably having a thicker crust beneath it than beneath the land.

We see, moreover, that the geological doctrine of absolute uniformity cannot be true as to vulcanicity, any more than it can for any other energy in play in our world. Its development was greatest at its earliest stages, when the great masses of the mountain chains were elevated. It is even now—though as compared to men's experience, and even to all historic time, apparently uniform and always the same—a decaying energy.

4. *On Ohio and other Gas Wells;* by Prof. J. S. NEWBERRY.—This popular record of the facts connected with gas wells was prepared for the Cleveland Herald by Prof. Newberry.

The many enquiries addressed to me in regard to the possibility of obtaining gas at Cleveland, by boring for it, indicates some interest in the subject, and prompts me to send you the following *résumé* of "what I know about our gas wells." Carburetted hydrogen is the chief component of the gas which escapes from the earth in wells and springs in many localities. It is evolved in the working of coal mines and constitutes the "fire-damp." It is also a constant associate of petroleum, and always issues in greater or less quantity from oil wells. It is given off, too, in the decomposition of recent vegetable matter, and may be seen bubbling up through the water of all pools in which plants are decaying. When it escapes from the earth it may be generally traced to beds of bituminous matter, such as coal, lignite, carbonaceous shale, asphalt, oil, etc. From these substances it may be obtained by artificial distillation, and is evolved by the spontaneous decomposition which all organic substances suffer on exposure.

As carburetted hydrogen produces a brilliant light in combustion, it is largely manufactured for the illumination of cities and residences. So extensively is it employed for this purpose that it may be regarded as an indispensable element to our modern civilization. It is not strange, then, that efforts have been made to utilize the immense quantities of gas which flow from wells and springs in so many different countries. The Chinese have for hundreds of years used for lighting and heating the gas which emanates from the earth in several provinces of their country. In the United States the gas which issues from the salt wells of the Kanawha Valley has been for many years employed as a fuel in the evaporation of the brine.

The town of Fredonia, in Western New York, has for more than forty years been fully or partially lighted by gas which issues from springs at that place. In the borings made for oil in the various oil districts of the Western States, the gas which has been produced so abundantly has been regarded as a useless, frequently an inconvenient and dangerous product. Within a year or two past, however, this gas has been utilized in numerous localities, and already a large number of wells have been bored for the express purpose of obtaining it. In some cases these gas wells have been highly productive, furnishing an abundance of material for heating and lighting in its most convenient and manageable form, so that this deserves to be reckoned as one of the important elements in the mineral resources of our country. As this method of procuring carburetted hydrogen gas forms a new industry, and one which will probably assume considerable importance, a few words in reference to its present condition and prospects may not be without interest to the public. I therefore extract from my notes a few facts in regard to some of the most interesting of our gas producing districts. On the Upper Cumberland, in Kentucky, gas accumulates in such quantities beneath the sheets of Lower Silurian limestone, that many hundred tons of rock and earth are sometimes blown out with great violence. These explosions have received the local name of "gas volcanoes." In Ohio gas escapes from nearly all the wells bored for oil in the oil-producing districts. Of these, two bored by Peter Neff, Esq., near Kenyon College in Knox county, present some remarkable features. These wells were bored in 1866, at the same geological horizon as that which furnishes the oil on Oil Creek, Pa. At the depth of about 600 feet, in each well, a fissure was struck from which gas issued in such volume as to throw out the boring tools and form a jet of water more than 100 feet in height. One of these wells had been tubed so as to exclude the water, and gas has continued for five years to escape from it in such quantity as to produce, as it rushes through a two and a half inch pipe, a sound that may be heard at a considerable distance. When ignited the gas forms a jet of flame three feet in diameter and fifteen feet long. The other well, which has never been tubed, constantly ejects, at intervals of one minute, the water that fills it. It thus forms an intermittent fountain one hundred and twenty feet in height. The derrick set over this well has a height of sixty feet. In winter it becomes incased in ice, and forms a huge translucent chimney, through which at regular intervals of one minute a mingled current of gas and water rushes to twice its height. By cutting through this chimney at its base, and igniting the gas in a paroxysm, it affords a magnificent spectacle—a fountain of water and fire which brilliantly illuminates the ice chimney. No accurate measure has been made of the gas escaping from these wells, but it is estimated to be sufficient to light a large city.

At West Bloomfield, N. Y., is another gas well, not unlike those I have described. This is bored to the depth of five hundred feet, reaching down to the vicinity of the Marcellus bituminous shales. From some measurements made by Prof. Wurtz, it appears that about fifteen cubic feet of gas escapes from this well every second. It is proposed to utilize this large amount of valuable combustible by conducting it through pipes to Rochester, a distance of twenty miles.

At Erie, Pennsylvania, there are now twenty-five wells in successful operation, most of which have been bored for the special purpose of obtaining gas. Mr. Henry Newton, my assistant in the School of Mines, has recently made, at my request, a careful examination of all of these wells, and has given me a detailed description of each. I quote one or two of these for the purpose of conveying a clear idea of their general character.

1st. H. Jarecki & Co. (Petroleum Brass Works) have two wells; the first bored for oil in 1864, 1,200 feet deep. No oil was obtained, but brackish water and an abundant supply of gas. This is used to light a few houses. The second well was bored in March, 1871, for gas, is 700 feet, deep and is used to supply gas to light the shop and heat the boilers. The supply is not at all regular, and has perceptibly failed since the Conrad well was sunk near it. The heating power of the gas from well No. 2 is roughly estimated at from eight to ten tons of coal per month.

2d. Brevellier's well was sunk for oil in 1864, depth 625 feet, diameter five inches. The gas supplies five fires in the soap factory and three in the house of the proprietor, besides lighting both establishments. It has been used in the factory for five years. * * *

At Conneaut and Painesville, Ohio, wells have been bored for gas with entire success, and others are being bored in these localities, and at many points further west.

In the vicinity of Cleveland, as in many other localities in the eastern half of the State, gas and oil springs are frequently met with, and wells have been bored for one or the other of these useful articles. Here, as elsewhere, there are two marked lines of gas and oil springs, connected with the outcrops of the two sheets of bituminous shale which underlie the surface: 1st, the Cleveland shale of Lower Carboniferous age, which crops out along the base of the hills that bound the Cuyahoga Valley. This bituminous shale is from thirty to sixty feet in thickness, and is the source of the oil of the East Cleveland and Kingsbury quarries, the noted gas spring at the brick-yard beyond East Cleveland, etc. The oil of Mecca and Liverpool is derived from the Cleveland shale. 2d, the Huron or Great Black shale, which passes beneath Cleveland, and rising westward comes to the surface in Huron and Erie counties, and forms a broad belt of outcrop, thence to the Ohio river. The Huron shale has a thickness of 300 to 400 feet, mostly black and highly bituminous, and is the source from which the gas of the Neff wells and the wells of the lake shore and the oil of Oil Creek are derived. In the valley of the Cuyahoga, in both Cuyahoga and Summit counties, a large number of wells were bored for oil some years since. Most of these yielded both gas and oil, but neither in large quantity. From a similar well in the valley of Rocky river, a copious flow of gas has continued to escape for several years. * * *

From these facts it will be seen that the flow of gas from wells bored in Cuyahoga county has been, on the whole, considerably less than from those farther east, and scarcely sufficient to warrant any great confidence in the success of experiments of this kind.

Since the geological formation at Cleveland is precisely the same as at Painesville, Conneaut and Erie, the difference noticeable in the quantity of gas obtained by boring in these localities has excited some surprise. The solution of the problem is, I think, this: As we approach the Alleghanies we find the strata which underlie Eastern Ohio and Western Pennsylvania more disturbed and broken. * * *

Nature and Uses of the Gas obtained from the Gas Wells.—The illuminating power of the gas of the West Bloomfield well and that from the wells of Fredonia and Erie has been measured, and found to be about one-half that of the gas used in most of our cities; or, technically, it is 7- or 8-candle gas, our city gas ranging from 14- to 18-candle power. It has usually the odor of petroleum, and contains a small quantity of condensible petroleum vapor. It is heavier than common street gas, and its heating power is greater. It contains carbonic acid and carbonic oxide, and when these are removed by passing it through a purifier its illuminating power is increased and its heating power diminished. All who use it speak of it in terms of high praise as being extremely convenient for heating and lighting, and without objectionable qualities. When delivered from the well directly to the burners and not consumed, it accumulates so that the pressure sometimes bursts the pipes. A steam guage applied to one of Mr. Neff's wells, in Knox county, ran rapidly up to 180 pounds, the highest pressure it would indicate. The pressure of the gas in the bottom of the well not tubed is certainly much greater than this, as it lifts a column of water not less than 500 feet in height. The gas of the well has no offensive odor, which is regarded as an excellence, but which may be a source of danger in its use. The offensive odor of street gas betrays its presence at once, reveals leaks and prevents accumulations and explosions, so that it may be regarded as almost indispensible to its safe use. The value of a well producing a copious flow of gas can hardly be overestimated. If the wells of Mr. Neff had been so located that their enormous product could have been utilized, they would have yielded larger pecuniary returns to their owner than any oil well has ever done.

The comfort and elegance imparted to an establishment by an abundant flow of odorless gas are well shown in the residence of General Casement. Here every fire in the house, in the kitchen range as well as the parlor grate, is fed by a fuel which gives a brilliant, cheerful flame, is supplied and cut off by turning a stop cock, makes little smoke and leaves no ashes; and all this in addition to an abundant supply for illumination. So great a luxury as this is makes doubly fortunate the man who possesses it, and is certainly worth some trouble and expense to those who would enjoy it. * * * *

5. *Fossil Birds from the Cretaceous of North America.*—In Dr. Coues' "Key to North American Birds," recently published, there is an Appendix on the fossil forms, prepared by Prof. O. C. Marsh, who has described nearly all the known species. From the Tertiary and Post-tertiary deposits of this country, eighteen extinct birds are enumerated; while in the Cretaceous beds no less than thirteen species have been discovered. The latter are of special interest—as but two other Cretaceous birds are known—and hence the list is repeated here, with additions up to the present time.

GRALLATORES.

Telmatornis priscus Marsh. This Journal, xlix, 210, March, 1870.

A species about as large as the King Rail (*Rallus elegans*), and probably allied to the *Rallidæ.* From the Cretaceous formation. Found near Hornerstown, New Jersey, and preserved in the museum of Yale College.

Telmatornis affinis Marsh. This Journal, xlix, 211, March, 1870.

A somewhat smaller species from the same formation and locality. Also in the Yale museum.

Palæotringa littoralis Marsh. This Journal, xlix, 208, March, 1870.

About equalling a Curlew in size. The remains were found in the Cretaceous green-sand, at the above mentioned locality, and are now preserved at Yale College.

Palæotringa vetus Marsh. This Journal, xlix, 209, March, 1870.

A smaller species, from the same formation, found at Arneytown, New Jersey. The known remains are in the Philadelphia Academy.

Palæotringa vagans Marsh. This Journal, iii, 365, May, 1872.

Intermediate in size between the two preceding species. Discovered in the same formation, near Hornerstown, New Jersey; and now in the museum of Yale College.

NATATORES.

Graculavus velox Marsh. This Journal, iii, 363, May, 1872.

This bird was related to the Cormorants, and was rather smaller than *Graculus carbo.* The remains were found in the green-sand of the Cretaceous formation, near Hornerstown, New Jersey, and are now at Yale College.

Graculavus pumilus Marsh. This Journal, iii, 364, May, 1872.

A smaller species, from the same formation and locality. The remains are in the Yale museum.

Graculavus anceps Marsh. This Journal, iii, 364, May, 1872.

Apparently a species of Cormorant, about as large as *Graculus violaceus.* From the Cretaceous of Western Kansas. Remains in the Yale College museum.

Graculavus agilis Marsh, sp. nov. This Journal, v, March, 1873.

A somewhat smaller species, probably of the same genus. The metacarpal bones are more slender, and the carpal fossa is wanting. From the same formation and locality. Remains, also in the Yale museum.

Hesperornis regalis Marsh. This Journal, iii, 360, May, 1872.

This bird was a gigantic Diver, related to the *Colymbidæ*. The skeleton measured about five feet nine inches in length. The known remains were found in the upper Cretaceous shale of Western Kansas, and are now in the Yale museum.

Laornis Edvardsianus Marsh. This Journal, xlix, 206, March, 1870.

This species was nearly as large as a Swan. The remains were discovered in the Middle Marl bed, of Cretaceous age, at Birmingham, New Jersey, and are now in the museum of Yale College.

Sub-class ODONTORNITHES, or AVES DENTATÆ.

Order ICHTHYORNITHES.

Ichthyornis dispar Marsh.—This Journal, iv, 344, Oct., 1872; 406, Nov., 1872; v, 161, Feb., 1873.

A bird about as large as a Pigeon, and differing from all known birds in having *teeth* and *biconcave vertebræ*. The known remains were found in the Upper Cretaceous shale of Kansas, and are preserved in the museum of Yale College.

Apatornis celer Marsh; (*Ichthyornis celer* Marsh).—This Journal, v, 74, Jan., 1873; 163, Feb., 1873.

A species about the same size as the above, but of more slender proportions. From the upper Cretaceous shale of Kansas, and now in the Yale College museum.

6. *Note on the Cretaceous of Wyoming;* by Prof. COPE, (slip from the Proc. Phil. Soc. Philadelphia, published on February 7th).—Prof. Cope made some remarks on the geology of Wyoming, especially with reference to the age of the coal series of Bitter Creek. He said that the discovery of the Dinosaur *Agathaumas sylvestris* had settled the question of age, concerning which there had been much difference of opinion, in favor of the view that they constitute an upper member of the Cretaceous series. In the sections made, he had succeeded in tracing the line of demarcation between these and the lower beds of the Green River epoch, and had found the leaf beds of the former to be immediately covered by deposits of mammalian remains, with an interval of a few feet only. In the same way, the close approximation of the Evanston Cretaceous coal to Tertiary strata was determined by the finding of numerous mammalian and reptilian remains in the lower part of the Wahsatch beds of Hayden, or even in the sandstones overlying the coal. Here two species of *Bathmodon* were found, corresponding with the nearly allied genus *Metalophodon* from the Bitter Creek locality. So far as is yet known, the *Bathmodontidæ* are diagnostic of the Green River formation, and on this and other grounds the Wahsatch beds of Evanston were regarded

as belonging to it. A further extension of the Green River formation was found at a point 400 miles westward (*see* Proc. Am. Philos. Soc., July, 1872), near Elko, Nevada, where fishes and insects occur in thin shales. Some of the former are nearly allied to species from the fish beds of Green River.

[Prof. Cope follows his statements on these points by others taking exception to an editorial note in this Journal, III, iv, 489, where, after mentioning Prof. Cope's determination of the Cretaceous age of the Bitter Creek coal, through the discovery of the remains of a Dinosaur, we remarked that "Prof. Cope was doubtless not aware that Mr. Meek had, in 1871, referred Dr. Hayden's collection from this formation on Bitter Creek, at Point of Rocks, to the Cretaceous." Prof. Cope now urges that Mr. Meek's determinations of the fossils were given with an expression of doubt, and that this was the ground of his making no reference to his opinions. It is nevertheless true, as we before stated, that Mr. Meek referred the beds mentioned to the Cretaceous without a query, expressing a doubt only with regard to the identification of the species with certain California forms; and this being a fact, we were not disposed to regard it as an intentional neglect on the part of Prof. Cope, as he would now have us to understand. We may add, that Prof. Cope is wrong in saying that "all his [Meek's] Coalville Cretaceous species are marked with a question;" for there are two species of *Inoceramus* and two of *Cardium*, in the list referred to (Hayden's Rep., 1871, p. 376), which are given without any indication of doubt. Further, the Cretaceous coal beds containing Dinosaurian remains, found by Prof. Marsh on Brush Creek, instead of being from 150 to 200 miles distant from the Bitter Creek saurian locality, are less than 75 miles.—EDS.]

7. *Recent Explorations in Vancouver and Queen Charlotte Islands.*—During the past summer Mr. J. Richardson, of the Geological Survey of Canada, has made unusually large collections of the fossils, minerals and other objects of interest from Vancouver and Queen Charlotte, a number of which were exhibited at the meeting of the Natural History Society of Montreal, Jan. 27th.

Mr. A. R. C. Selwyn said that these collections establish conclusively the fact that the coal fields of the two islands belong to the same geological horizon. In each case the coal fields are of the same age as the chalk formation of Europe and elsewhere. Further, the coal of Queen Charlotte Island is found to be a true anthracite in formations, as new as the chalk. The coal seams of Vancouver' rest directly upon crystalline rocks, in which limestones predominate. Associated and interstratified with the limestones are diorite, and what seem to be epidotic and chloritic rocks. At Horn Lake (in Vancouver Island) a wall of limestone, interstratified with hornblende and dioritic rocks, rises almost perpendicularly to a height of about 1,000 feet. As this cliff is near the proposed line of the Pacific Railway, Mr. Richardson thinks that it will be of value for lime-making and building purposes; it is also a fine statuary marble. The same gentleman estimakes the Comox coal field in Vancouver, to have an area of 300 square miles. It is underlaid by coal seams of from two to ten feet in thickness, which would probably yield an average of 11,[illegible] tons per square mile. The total production of this field, to a depth of [illegible] feet from the surface, is computed to be about [illegible] millions of tons. The Nanaimo coal field has an estimated area of [illegible] square miles, and contains three or more seams of from three to ten feet in thickness. There are also large areas of coal-bearing rocks to the southeast of Nanaimo, and on the extreme northern part of Vancouver, as well as on the mainland, near the mouth of the Fraser river.

Principal Dawson said that although the vegetation of the Carboniferous period was strikingly different from that of the Vancouver and Queen Charlotte coal seams, the physical conditions under which the coal was formed, was similar in both cases. In other words, coal seems to have been produced from the vegetation of swamps and forests, at different geological periods, and from very dissimilar kinds of plants. In the coal of Vancouver leaves were found which the lecturer said belong to species of oak, plane and poplar. The coal of Vancouver is bituminous, while that of Queen Charlotte is ascertained to be anthracite. Anthracite, Dr. Dawson went on to say, is probably bituminous coal altered by heat; it presents an appearance as if it had been baked. Very few traces of plants have been found in the Queen Charlotte anthracite, and this was attributed to the metamorphosed or altered nature of the coal. Specimens of fossil wood were, however, found, not in the anthracite, but in beds associated with it which Mr. Richardson believes to be of the same age. These woods are allied to those members of the yew family which grow at the present day on the west coast of America and on the east coast of Asia. They are all fragments of cone-bearing trees, and these, from their resinous nature, resist decay better than other kinds of wood. In some of these pieces, traces of boring molluscs allied to the Shipworm of modern seas, may be seen. Another specimen shows the leaf of a fern associated with marine shells. In the first case the wood must have been waterlogged, and the fern had probably been washed out to sea. Specimens of fossil fruits, from Queen Charlotte, were exhibited. These were stated to be probably allied to certain fossil fruits described from the Isle of Sheppey, which were once thought to belong to true palms, but which are now assigned a place between the palms proper and the screw pines. Some curious concretions from the bituminous coal of Vancouver were also exhibited.

Mr. Billings gave a description of the distribution of the cretaceous rocks of North America, and then called attention to some of the characteristic fossils of the Vancouver and Queen Charlotte strata. Among these were large Ammonites, Nautili and various marine shells, of the same genera for the most part as those which are frequent in the European chalk formation. He remarked that in the present collection he had not detected any remains of large reptiles, or any sea urchins, both of which are common in the Cretaceous rocks of other localities.

Mr. Whiteaves also made some remarks on the fossils collected. He said that certain specimens from a single locality in Queen Charlotte indicated a higher geological horizon than the chalk, and that he was disposed to refer them to older Tertiary rocks, probably of Eocene date.—*Montreal Gazette*, Jan. 30.

8. *Cainozoic* versus *Cœnozoic or Cenozoic.*—We observe that Lyell, in his geological works, even the most recent, uses the word *Cainozoic* instead of *Cœnozoic* or *Cenozoic.* Why the propounder of the terms Eocene, Miocene, etc., should thus spell the word is incomprehensible. If he is right in it, then to be consistent, he ought to say *Eocain*, *Miocain*, *Pliocain*, *Post-pliocain*; for all have the same root *καινος.* And further, consistency, which we all love, will not allow us to stop here. We must go on, and say *Palaiozoic*, not Palæozoic or Paleozoic; *aithiopic*, not ethiopic; *aither*, not æther or ether; *aisthetic*, not esthetic; *paian*, not pæan; we must call demons, *daimons*, and pedagogues, *paidagogues*, and speak of the earth as a *sphaire* in space, no longer as a sphere; and all this to put *cain* into the designation of the last great geological era. If successful, it would be a poor object secured at a disadvantage. J. D. D.

9. *Third and Fourth Annual Reports of the Geological Survey of Indiana, made during the years* 1871 *and* 1872; by E. T. Cox, State Geologist, assisted by Prof. John Collett, Prof. B. C. Hobbs, Prof. R. B. Warder, and Dr. G. M. Levette. pp. 488, 8vo. Indianapolis, 1872.—This volume, from the Geological Survey of Indiana, is occupied mainly with facts connected with its local geology, and especially the Coal formation. The State geologist reports on Perry county, and briefly on Harrison and Crawford counties; Prof. Collett on Dubois, Pike, Jasper, White, Carroll, Wabash, Miami and Howard counties; Prof. Hobbs on Parks county; and Prof. Warder on Dearborn, Ohio and Switzerland counties—the report contains a large number of valuable analyses of mineral coal of the State—a product that is attracting much attention—the economical value of the various coals being determined by experiment and calculation, as well as the products of combustion; and for some kinds complete ultimate analyses are given. The volume is accompanied by a case in 8vo, containing three maps. The long list of errata at the end shows that the State printing office did not use all the care it might to secure accuracy.

10. *A Manual of Paleontology*, for the use of students, by Henry Alleyne Nicholson, M.D., Professor of Natural History in University College, Toronto. 8vo, 601 pp. Blackwood & Sons, Edinburgh and London, 1872.—This work will be found useful to the student of geology, as it supplies, in part, a want which has long been felt. The principles of Paleontology are first discussed, and a concise classification of the Animal Kingdom given. In Part Second, Paleontology proper is treated of in detail, special

prominence being given to the remains of invertebrates. The third part gives a general view of Paleobotany, and the remainder of the work is devoted to Stratigraphical Paleontology. The volume contains about 400 figures, most of them excellent wood-cuts, taken from D'Orbigny's Cours Elémentaire de Paléontologie, by arrangement with the publishers of that work.

11. *A Treatise on Building and Ornamental Stones of Great Britain and Foreign Countries*; arranged according to their geological distribution and mineral character, with illustrations of their application in ancient and modern structures. By Edward Hull, M.A., F.R.S., Director of the Geological Survey of Ireland. pp. 333, 8vo. London (McMillan & Co.).—Mr. Hull has made out of his subject an interesting and useful book, and we believe he is correct in saying it is the first attempt in our language to discuss this important department of practical geology and mineralogy in a separate treatise. The classification of his subject is well considered, as follows: Part II, Granitic Rocks, Syenite; Part III, Porphyritic Rocks; Part IV, Greenstone Rocks (Diorite, Diabase, Minette, etc.), Augitic Rocks (Basalt, Dolerite and Melaphyre), Lavas; Part V, Serpentinous Rocks; Part VI, Marble; Part VII, Alabaster; Part VIII, The Rarer Ornamental Stones; Part IX, Malachite; Part X, Calareous group of building stones; Part XI, Sandstone group of building stones; Part XII, Tufaceous and volcanic building stones; Part XIII, Slates. The concluding observations (Part XIV) are on the physical and chemical characters of building stones; on the selection of building stones, with special reference to climate and the nature of the atmosphere; and the weights per cubic foot of British building stones. The author presents a large number of interesting data under each of these main divisions, subdividing his subject by a separate treatment for various countries, and bringing in under appropriate heads many data respecting the ornamental and fine arts, both ancient and modern. For the American reader this volume is naturally fuller upon British examples than is desirable, and is far too imperfect in its notices of American building materials, some of the most important of which are not mentioned. The geological reports of the various States, which are the chief sources of information on this subject, appear not to have been consulted by our author, while in general the book shows evidence of careful preparation by consultation of authorities.

12. *The History of Balanoglossus and Tornaria*; by Alexander Agassiz. Quarto with three plates. From the Memoirs of the American Academy of Arts and Sciences, vol. ix, p. 421, January 14, 1873.—In this very interesting and important memoir, Mr. Agassiz gives us a nearly complete history of the development of the larva long known as *Tornaria*, and until recently universally regarded as the larva of an *Echinoderm*, into the very remarkable worm *Balanoglossus*. That *Tornaria* is the larva of this or some allied genus, had been rendered very probable by the observations of Metschnikoff, published in 1870, as stated by Mr. Agassiz, but the evidence was not conclusive, for the complete

development and metamorphosis had not been observed. It is, therefore, very gratifying to have this important point settled so soon and so satisfactorily. Mr. Agassiz gives an excellent account of the structure, both of the larva in its various stages and of the adult *Balanoglossus*, and discusses their relations to other worms. He concludes that when, at last, the true structure of this remarkable larva and its history have been ascertained, its true relations to the larvæ of Annelids are sufficiently clear, while the relations to Echinoderm larvæ are not so close as had been supposed, for the parts are not homologous with those of the latter, although the external resemblance is quite remarkable. Nor does he admit that this worm, either by its structure or mode of development, can be regarded as connecting the Annelids and Echinoderms. "It is undoubtedly the strongest case which could be taken to prove their identity; but when we come carefully to analyze the anatomy of true Echinoderm larvæ, and compare it with that of Tornaria, we find that we leave as wide a gulf as ever between the structure of the Echinoderms and that of the Annuloids." The plates are excellent and illustrate well both the external appearance and anatomy of the Tornaria-stage, the young *Balanoglossus*, and the adult.

This worm is of large size when mature, and lives in the sand at low-water mark. It occurs on the sandy shores of southern New England and southward; Mr. Agassiz has found it at Beverly, Mass., as well as on the shores of Vineyard Sound and at Newport. The writer also has specimens from Naushon Island.

Mr. Agassiz does not mention, and therefore has doubtless overlooked the fact, that Mr. Chas. Girard, just twenty years ago, described a species of *Balanoglossus* from South Carolina, under the name of *Stimpsonia aurantiaca.** It is true that Girard's description was quite imperfect, like all the early descriptions of this singular genus, but no one can doubt that his species was a *Balanoglossus*, and judging from the description, it is most likely identical with the *B. Kowalevskii*, so well described and illustrated by Mr. Agassiz in the memoir before us. A. E. V.

13. *Journal of Researches into Natural History and Geology of the Countries visited during the voyage of H. M. S. Beagle round the world, under command of* Capt. FITZROY, R. N. By CHARLES DARWIN, M.A., F.R.S., author of Origin of Species, etc. New edition, pp. 519, 12mo. New York, 1871. (D. Appleton & Company).—With what pleasure this work was read more than thirty years ago, when it first made its now famous author a familiar name to all naturalists and lovers of nature, only the elder portion of our readers can remember. It remains a classic among journals of scientific travel, as charming to-day as ever, and with the advantage of a few later touches by the author.

14. *Note on the Dates of some of Prof. Cope's recent Papers*, by O. C. MARSH.—The Proceedings of the American Philosophical Society, vol. xii, No. 89, just issued (Feb. 6th, 1873), contain

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* Proceedings of the Academy of Natural Sciences of Philadelphia, vol. vi, p. 367, 1853.

several communications from Prof. Cope on vertebrate fossils from Wyoming. There are some errors with regard to the dates, bearing the same way with those pointed out on pp. 118 and 122, which should be corrected. In the table of contents of this number, under the stated meeting, August 15th, 1872, eight papers by Prof. Cope are enumerated; and it might be inferred that they were read on that day. In fact, however, there was no meeting of the Society on the 15th, the regular August meeting having been held Friday, August 16th, at which three only of these papers were read by title, or entered on the records. At the next regular meeting, September 20th, 1872, five papers by Prof. Cope were announced, or read by title. But as now published in the Proceedings, four of these purport to have been read September 19th, 1872, when no meeting was held on that date. The actual publication of these papers, by distribution is of course a distinct matter, and the evidence is conclusive that none of these were so published before Oct. 20th, 1872, and some of them not until long after.

III. ASTRONOMY.

1. *Researches in Spectrum Analysis in connection with the Spectrum of the Sun.* No. I.; by J. NORMAN LOCKYER, F.R.S.—The author, after referring to the researches in which he has been engaged since January, 1869, in conjunction with Dr. Frankland, refers to the evidence obtained by them as to the thickening and thinning of spectral lines by variations of pressure, and to the disappearance of certain lines when the method employed by them since 1869 is used. This method consists of throwing an image of the light-source to be examined on the slit of the spectroscope.

It is pointed out that the phenomena observed are of the same nature as those already described by Stokes, W. A. Miller, Robinson, and Thalen, but that the application of this method enables them to be better studied, the metallic spectra being clearly separated from that of the gaseous medium through which the spark passes. Photographs of the spark, taken in air between zinc and cadmium and zinc and tin, accompany the paper, showing that when spectra of the vapors given off by electrodes are studied in this manner, the vapors close to the electrode give lines which disappear from the spectrum of the vapor at a greater distance from the electrode, so that there appear to be long and short lines in the spectrum.

The following elements have been mapped on this method:—Na, Li, Mg, Al, Mn, Co, Ni, Zn, Sr, Cd, Sn, Sb, Ba, and Pb, the lines being laid down from Thalen's maps, and the various characters and lengths of the lines shown.

In some cases the spectra of the metals, enclosed in tubes and subjected to a continually decreasing pressure, have been observed. In all these experiments the lines gradually disappear as the pressure is reduced, the *shortest lines disappearing first and the longest lines remaining longest visible.*

Since it appeared that the purest and densest vapor alone gave the greatest number of lines, it became of interest to examine the spectra of compounds consisting of a metal combined with a non-metallic element. Experiments with chlorides are recorded. It was found in all cases that the difference between the spectrum of the chloride and the spectrum of the metal was that under the same spark-conditions all the short lines were obliterated. Changing the spark-conditions, the final result was that only the very longest lines in the spectrum of the metallic vapor remained. It was observed that in the case of elements with low atomic weights, combined with one equivalent of chlorine, the numbers of lines which remain in the chloride is large, 60 per cent, *e.g.*, in the case of Li, and 40 per cent in the case of Na; while in the case of elements with greater atomic weights, combined with two equivalents of chlorine, a much smaller number of lines remain—8 per cent in the case of barium, and 3 per cent in the case of Pb.

The application of these observations to the solar spectrum, to elucidate which they were undertaken, is then given.

It is well known that all the known lines of the metallic elements on the solar atmosphere are not reversed. Mr. Lockyer states what Kirchoff and Angström have written on this subject, and what substances, according to each, exist in the solar atmosphere. He next announces the discovery that, with no exception whatever, *the lines which are reversed are the longest lines.* With this additional key he does not hesitate to add, on the strength of a small number of lines reversed, zinc and aluminium (and possibly strontium) to the last list of solar elements given by Thalen, who rejected zinc from Kirchoff's list, and agreed with him in rejecting aluminium. It need scarcely be added that these lines are in each case the longest lines in the spectrum of the metal.

The help which these determinations afford to the study of the various cyclical changes in the solar spectra is then referred to.—*Phil. Mag.*, Feb., 1873.

IV. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *Explorations west of the 100th meridian.*—Under this title the U. S. Engineer Department has inaugurated the systematic exploration of the unmapped portions of our territory, and one season of field-work has already been accomplished. A plan has been elaborated for the publication of the geographical results in the form of an atlas. Upon a skeleton map the territory is divided, by certain parallels and meridians, into eighty-five approximately equal rectangles, each of which will be represented by a single page of the completed atlas, and the pages will be progressively compiled as the work proceeds. The scale adopted is —8 miles= 1 inch, and the size of each plate will be about 18×23½ inches. The work for 1872 was intrusted to Geo. M. Wheeler, 1st Lieutenant of Engineers, and his corps of topographers is now engaged in the construction of maps, which shall combine the data accumulated last year with the results of previous explorations by the same officer in 1869 and 1871. The field-work of the

last season occupied the party from July 15th to Dec. 10th, and covered an area of 41,000 square miles in southwestern Utah and adjacent portions of Nevada and Arizona. Connection was made at the north with Clarence King's survey of the fortieth parallel, at the east with the explorations of Prof. J. W. Powell, and at the south and west with Lieut. Wheeler's work of previous years. The Sevier river was explored from its numerous heads to its mouth in Sevier lake, and the identity of the latter with the Preuss lake of maps was established; its salinity was found somewhat less than that of Great Salt lake. The limits of the Sevier basin were traced, and especially the high divide that separates it from the Virgen, Kanab and Paria; and those last mentioned streams—tributaries of the Colorado of the West—were surveyed to their mouths. Primary astronomical stations were made at Pioche in Nevada, at Gunnison and Beaver in Utah, and at Fort Steele, Laramie and Cheyenne in Wyoming. Examinations were made of the silver mining districts so rapidly springing into existence in western Utah, and of the coal field of southern Utah; and specimens and other material were accumulated in all departments of geology and natural history.

The scientific members of the party were H. C. Yarrow, M.D., naturalist, assisted by H. W. Henshaw; G. K. Gilbert, geologist, assisted by E. E. Howell; M. S. Severance, ethnologist; and Messrs. J. H. Clark, E. P. Austin and W. W. Maryatt, astronomers, all of whom, excepting Mr. Austin, are now engaged at Washington in the preparation of the results of their labors for publication. A preliminary report will shortly appear.

2. *Instances of remarkably low temperature observed at New Haven, Conn.*—During the past winter the thermometer at New Haven has in two instances indicated a degree of cold so unusual that it has been thought desirable to place the facts on record in a permanent form, and also to institute a comparison with other known cases of remarkable cold. The meteorological record at New Haven commences with the Register of Pres. Ezra Stiles in June, 1778, and is continuous to the present time with the exceptions of a few months in 1779 and 1795. The following list is designed to include all the cases in which the thermometer at this place has been known to sink as low as ten degrees below zero. So much depends upon the location of the thermometer that it is impossible to institute a very close comparison between the temperatures in these different cases.

1. Jan. 5, 1835. Prof. Benjamin Silliman observed 23 degrees below zero; Dr. Alfred S. Monson 24°; Mr. Rodney Burton 24°; and Prof. C. U. Shepard 26°.
2. Jan. 30, 1873. Prof. W. A. Norton observed 23 degrees below zero; Prof. C. S. Lyman 24¾°; Prof. A. C. Twining 26°; Prof. W. H. Brewer 26°; Mrs. A. N. Skinner 26°.
3. Jan. 24, 1857. Dr. Alfred S. Monson observed 12° below zero: Mr. Hawley Olmsted 18°; and Mr. A. N. Skinner 22°.
4. Dec. 28, 1872. Prof C. S. Lyman observed 19½° below zero; Prof. B. Silliman 19½°; and Mrs. A. N. Skinner 20°.
5. Jan. 8, 1866. Prof. E. Loomis observed 18° below zero.
6. Jan. 25, 1821. Prof. A. M. Fisher observed 12° below zero; and Dr. Alfred S. Monson 17½°.

7. Feb. 7, 1855. Dr. Alfred S. Monson observed 8° below zero; and Mr. Rodney Burton 16°.
8. Feb. 1, 1826. Dr. Alfred S. Monson observed 15° below zero.
9. Jan. 4, 1835. Dr. Alfred S. Monson observed 15° below zero.
10. Jan. 31, 1826. Dr. Alfred S. Monson observed 14° below zero.
11. Jan. 5, 1841. Dr. Alfred S. Monson observed 12° below zero; Prof. C. S. Lyman 12°; and Mr. E. C. Herrick 14°.
12. Jan. 11, 1859. Dr. Alfred S. Monson observed 7° below zero; and Mr. Joseph Bennett 14°.
13. Feb. 3, 1783. Pres. Ezra Stiles observed 13° below zero.
14. Jan. 6, 1835. Dr. Alfred S. Monson observed 13° below zero.
15. Feb. 2, 1789. Pres Ezra Stiles observed 12° below zero.
16. Feb. 15, 1817. Pres. J. Day observed 12° below zero.
17. Dec. 16, 1831. Dr. Alfred S. Monson observed 11° below zero.
18. Jan 18, 1840. Dr. Alfred S. Monson observed 11° below zero.
19. Feb. 10, 1784 Pres. Ezra Stiles observed 10° below zero.
20. Jan. 10, 1797. Dr. Isaac Beers observed 10° below zero.
21. Jan. 19, 1821. Prof. A. M. Fisher observed 5° below zero; and Dr. Alfred S. Monson 10°.
22. Jan. 26, 1821. Prof. A. M. Fisher observed 5° below zero; and Dr. Alfred S. Monson 10°.
23. Jan. 5, 1822. Dr. Alfred S. Monson observed 10° below zero.
24. Dec. 13, 1825. Dr. Alfred S. Monson observed 10° below zero.
25. Jan. 10, 1859. Dr. Alfred S. Monson observed 4° below zero; and Mr. Joseph Bennett 10°.

It thus appears that the thermometer at New Haven has fallen 10° below zero 25 times in 95 years. Of these cases 12 per cent occurred in December, 64 per cent in January, and 24 per cent in February; and all are embraced within the limits of Dec. 13th and Feb. 21st, a period of 70 days. E. L.

3. *Tables and Results of the Precipitation in Rain and Snow in the United States, and at some stations in adjacent parts of North America, and in Central and South America*; collected by the Smithsonian Institution, and discussed, under the direction of Joseph Henry, Secretary, by Charles Schott, Assistant U. S. Coast Survey. 174 pp., 4to, with several plates and charts. Smithsonian Contributions to Knowledge, No. 222.—Prof. Henry, in his preface, makes the following statement as to the sources of the material used by Mr. Schott. "The following memoir contains in tabulated form the abstracts of all the records of observations of the rain-fall which have been made from the early settlement of this country down to the close of the year 1866, so far as they could be obtained. These records are from about one thousand two hundred stations, and consist of the observations made under the direction of the Smithsonian Institution, assisted since 1854 by the Patent Office and Department of Agriculture; of those by the Medical Department of the United States Army, of those by the United States Survey of the North and North-west Lakes, of those made by the New York University System, by the Franklin Institute of Philadelphia, and also of those by other scientific institutions and individuals. For a more definite account of the various sources of information we would refer to subsequent pages. It is proper, however, that we should here express our obligations for the valuable coöperation of the Medical Depart-

ment of the Army under Surgeon-General Barnes, who has given us free access to all the unpublished records, and also for that of the Department of Agriculture under the Commissioner, General Capron."

The discussion of the observations by Mr. Schott is very elaborate, and brings out many important results. More than a hundred pages are occupied with tables of the precipitation over various parts of the United States. Following these there are chapters containing generalizations of the tabular results, and remarks on the construction of rain charts in the United States, with deductions as to annual fluctuations, and secular changes in the rainfall. The work is illustrated by a large number of diagrams in the text, and in plates I to V; and also by three charts, showing with much detail the geographical distribution, over the United States, of rain (and melted snow), severally for the whole year, the summer season, and the winter.

4. *Results of a Series of Meteorological Observations made under instructions from the Regents of the University at sundry stations, in the State of New York.* Second Series, from 1850 to 1863, inclusive; with records of rainfalls and other phenomena, to 1871, inclusive. Prepared from the original returns by FRANKLIN B. HOUGH. 406 pp., 4to. Albany: Published by Legislative Authority.—For this extensive and valuable contribution to American Meteorology, science is indebted to the liberality of the State of New York, and the judgment and care of Mr. F. B. Hough. Besides tables of ordinary meteorological observations, it contains a long list of displays of the aurora borealis, with some observations on meteors and shooting stars.

5. *On the Dardanelles and Bosphorus Under-current;* by WM. B. CARPENTER.—It will be in the recollection of such of your readers as have followed the discussion on Ocean Currents, that I ventured nearly two years ago* to predict the existence of an under-current of dense Ægean water into the Black Sea, "on the double ground of *à priori* and *à posteriori* necessity;"—that is, I affirmed it to be a necessary result of the excess of specific gravity in the water of the Ægean above that of the Euxine; whilst, I argued, if the salt continually passing out of the Black Sea by the surface-current were not thus replaced, the continual excessive influx of river water would, in time, wash the whole of the salt out of its basin. * * *

Having understood that the *Shearwater*, on the completion of the survey of the Gulf of Suez, would proceed to the Dardanelles, I requested the Hydrographer to direct that the question of the under-current should be thoroughly examined; and he issued instructions accordingly.

I yesterday learned through the *Levant Herald:*—(1) that the existence of a strong under-current has been placed beyond all question, a boat having been carried along by the "current-drag" suspended from it, *in opposition to the surface-current:* (2) that the rate of this under-current is estimated as greater than the speed of the *Shearwater's* steam-launch; and (3) that it runs at a depth of twenty fathoms,—precisely that at which my interpretation of Captain Spratt's experiments has led me to predicate its existence.

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* Proceedings of Royal Society, Dec. 8, 1870.

I venture to think that this verification of my prediction will be regarded as a confirmation of the general physical theory of under-currents on which it was based; and it is now for those who oppose that theory to show by what other force than the difference in the *weight* of the Ægean and the Black Sea columns, consequent upon their great difference in specific gravity, the Dardanelles under-current can be sustained.—*Nature*, Oct. 24.

6. *International Scientific Series.*—Under this title the Messrs. Appleton have announced the titles of some thirty works, by both American and European authors, upon a great variety of subjects, Physical, Chemical, Biological, Physiological, Zoological, Geological, etc. Among the authors are Wurtz, Sainte Claire Deville, Quetelet, Quatrefages and Berthelot from France and Belgium; Vichrow, of Berlin; Balfour Stewart, Bastian, Spencer, Odling, Lockyer, Ramsay, Sir John Lubbock, Carpenter and Huxley, in Great Britain; Dana, Whitney, (W. D.), Austin, Flint, Jr., and Johnson (S. W.), in America. The rights of the authors are protected and publication is insured in both England, France, Germany and the United States, by publishing houses of the highest respectability. It is the plan of this International Series to provide, from the best authors in all departments of science, essays or treatises which shall bring the topics which they treat to the level of all educated and intelligent people, thus rendering science popular in the only sense in which it is proper to use that term. It is a hopeful sign of the times that all the leading minds who have been invited to unite in this undertaking, have cordially agreed to co-operate in promoting its objects.

The first of this series is an essay by Prof. John Tyndall, entitled: *The Forms of Water, in Clouds and Rivers, Ice and Glaciers.* pp. 192, 12mo, 1872. The charm of Prof. Tyndall's style is its simplicity and directness, the ease with which he makes plain even abstruse propositions, and thus leads the reader on to the comprehension of topics far removed from ordinary apprehension. In the present work the author borrows his illustrations, of course, almost exclusively from his familiar walks in the Swiss Alps, leading the reader from clouds, rains and rivers, and the sun as their cause, to the architecture of ice, the genesis of glaciers, with all the questions of their motion, moraines, crevasses, molecular mechanism, dirt bands, erratic blocks, ancient glaciers, glacial theories, vegetation, cleavage, lamination, etc. The only regret of the reader is that the story is so soon told. B. S.

OBITUARY.

Mrs. Mary Somerville, distinguished for her attainments in Mathematics and works in that department, as well as in Physical Geography, and some other branches of Science, died on the 1st of December. She was born probably as early as the year 1780.

Reverend **Adam Sedgwick**, Woodwardian Professor of Geology in the University of Cambridge, one of the oldest of English geologist, and best of men, died on the 28th of January, aged 86 years. He was born at Dent in Yorkshire, June, 1784, and graduated at Trinity College, Cambridge, in 1808. The labors of no English geologist have made a more profound impression on geological science.

James Henry Coffin, Professor of Mathematics and Astronomy in Lafayette College, Easton, Pa., Feb. 6, 1873. Prof. Coffin was born Sept. 6, 1806 in Northampton, Mass., was educated by Rev. Moses Hallock, of that State, and graduated at Amherst, in 1838. His life has been spent in teaching. While professor in Williams College, from 1838 till 1843, he advised and directed the building of Greylock Observatory, on Saddle Mountain, the first combined self-registering anemometer and barometer being there placed by him, an improved duplicate of which he recently sent to the Brazilian Government. Since 1846 he has been connected with Lafayette College. Shortly before his death he completed a revised and enlarged edition of the "Winds of the Northern Hemisphere," published by the Smithsonian Institute in 1851. The most noteworthy of his other publications are "Solar and Lunar Eclipses," "A Discussion of a Meteoric Fire-ball," &c.

Matthew F. Maury, formerly commander in the United States navy, died at Lexington, Va., on the 1st of February. Professor Maury won widely extended fame for his investigations in regard to winds and ocean currents, and for varied astronomical and atmospherical observations, the results of which, as embodied in his numerous volumes, have been of great value to commerce. In 1844 he became superintendent of the depot of charts and instruments, at Washington, out of which have grown the Naval obserservatory and the hydrographic office of the United States. At the time of his death he was Professor of Physics in the Virginia Military Institution.

Prof. **Macquorn Rankine**, Professor of Civil Engineering and Mechanics in the University of Glasgow, died at his residence in that city, Christmas eve, 1872 in the fifty-third year of his age.

Annual Report of the Survey of the Northern and Northwestern Lakes, by C. B. Comstock, Major of Engineers, being Appendix Z of the Annual Report of the Chief of Engineers. pp. 81, 8vo.

Contribuzioni Mineralogiche per servire alla Storia dell' Incendio Vesuviano del mese di Aprile, 1872; per Arcangelo Scacchi. 36 pp., 4to, Napoli, 1872.

Sulla Origine della cenere Vulcanica; nota del A. Scacchi. 10 pp., 4to, 1872.

Notizie preliminari di alcune specie mineralogiche rinvenute nel Vesuvio dopo l'incendio di Aprile, 1872; nota del A. Scacchi. 4to.

Die küntslich dargestellten Mineralien. nach G. Rose's krystallo-chemischen Mineralsysteme geordnet; von Dr. C. W. C. Fuchs, Professor in Heidelberg. Eine von der Holländischen Gesellschaft der Wissenchaften in Haarlem, an Mai, 1871, gekrönte Preisschrift. pp. 174, 4to. Haarlem: 1872.

APPENDIX.

Discovery of another new Planet; by Dr. C. H. F. PETERS. (From a letter dated, Litchfield Observatory, Clinton, N. Y., Feb. 19, 1873.)

ANOTHER planet, the 130th of the group of asteroids, was found here night before last, and its place determined as follows:

1873. Feb. 17. $12^{h}\,3^{m}\,37^{s}$ m. t. $\alpha = 10^{h}\,0^{m}\,16^{s}{\cdot}30$ $\delta = +13^{\circ}\,30'\,31''{\cdot}5$

It shines as a star of the 11th magnitude. From the several comparisons I derive its motion in 24 hours to be: $\Delta\alpha = -45^{s}$ and $\Delta\delta = +9'\,11''$.

THE

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[THIRD SERIES.]

ART. XXVI.—*Comparison of the mean daily range of the Magnetic Declination and the number of Auroras observed each year, with the extent of the black spots on the surface of the Sun;* by ELIAS LOOMIS, Professor of Natural Philosophy in Yale College.

IN a former number of this Journal (Sept., 1870), I instituted a comparison between the mean daily range of the magnetic declination, and the number of auroras observed each year, and also with the extent of the black spots on the surface of the sun. That comparison appeared to me to establish a connection between these three classes of phenomena, and indicated that auroral displays at least in the middle latitudes of Europe and America are subject to the law of periodicity; that their grandest displays are repeated at intervals of about sixty years, and that there are also other fluctuations less distinctly marked which succeed each other at an average interval of about ten or eleven years, the times of maxima corresponding quite remarkably with the maxima of the solar spots.

These conclusions were based upon a combination of the auroral observations made in Europe south of the parallel of 55°, with the observations made at New Haven and Boston. It is of course desirable that conclusions so important should be tested by a comparison with all the materials which we can command; and I therefore improve the opportunity afforded by the publication of a new and very complete catalogue of auroras by Prof. Joseph Lovering. This catalogue is contained in the Memoirs of the American Academy, vol. x.

In comparing the relative extent of the black spots on the surface of the sun, I employ the relative numbers given by Dr. Rudolf Wolf of Zurich. While the correspondence between the fluctuations of the sun's spotted surface, and the range of the magnetic declination is very remarkable; some small discrepancies are noticeable, particularly before the year 1825, when the observations were less numerous and systematic then they have since been; and it has appeared to me that these discrepancies might be at least in part the result of the incompleteness and looseness of the observations themselves. In my former article already referred to, I pointed out certain years for which the relative numbers given by Dr. Wolf appeared to me to rest upon a very uncertain basis. These years were 1793, 1794 and 1795; and also the years 1801 to 1807 inclusive.

In the Vierteljahrsschrift, vol. xvi, pp. 89–99, Dr. Wolf has given in full the materials for these years, and has deduced new values for the relative numbers differing considerably from those which he had before published. For the years 1793, '4 and '5, it appeared to me that Dr. Wolf's relative numbers were too small. Dr. Wolf's new discussion of the observations has led him to increase his relative numbers for each of these years, by an average quantity equal to two-thirds of that which I had proposed, thus admitting the substantial justice of my criticism. It is also noticeable that the observations for the last month of 1794 and the first two months of 1795, furnish very large relative numbers, indicating an unusual activity upon the sun's surface at that time; and this is a fact to which in my former article I desired to call special attention.

For the years 1801–1807, the observations are very few and meager, and the comparison of Dr. Wolf's relative numbers with the range of the magnetic declination led me to think that his relative numbers were somewhat too large. Dr. Wolf's new discussion of the observations has led him to a slight increase instead of a diminution of his former numbers. The following statement will show how little weight is to be attached to these numbers. The year of supposed maximum in the extent of the solar spots, is 1804. Now for the year 1802, Dr. Wolf has only *eight* observations from which he is able to deduce a value for his relative number; for 1803, he has but *two* observations; for 1804, but *three;* for 1805, but *one;* and for 1806, but *four* observations.

Now for the year 1870, in which he had himself carefully observed the solar spots on 276 days, and had received observations from other astronomers which informed him of the condition of the sun's surface on 352 days of the year, he obtained 173·3 as the correct value of the relative number; or by a different mode of computation he might reduce this number to 169·1, and these are the values which he published in the Vierteljahrsschrift, vol. xvi, pp. 84–85. But it would seem as if he subsequently became alarmed at the discrepancy between these numbers and the range of the magnetic needle as reported from the observatories of Munich and Prague, and in the Vierteljahrsschrift, vol. xvii, pp. 2–6, he undertakes a new discussion of these observations and reduces the relative number for 1870 to 139·6. If, when his materials were so abundant as in 1870, he could, by a change in his mode of reduction, produce a difference of 30 in his relative number for the year, it seems quite allowable to conclude that for the years 1801–1807, during which we have extremely few observations, Dr. Wolf's relative numbers may require a still larger correction.

As, however, the main object of this article is to establish the periodical character of auroral displays, I prefer to take Dr. Wolf's relative numbers precisely as he has furnished them in his latest publications, without insisting upon any further modification, although I still think some modification is called for in a few cases. The following table shows the relative numbers representing the frequency and extent of the solar spots for each year since 1776, according to what I understand to be Dr. Wolf's latest determinations. I begin the comparison with the year 1776, because that is the year on which the observations of the magnetic needle commence. The relative numbers from 1784 to 1811 inclusive, are taken from the Vierteljahrsschrift, vol. xvi, p. 86; the others are taken from the Vierteljahrsschrift, vol. xiii, p. 121, with the exception of the last four years, which are taken from more recent volumes of the Vierteljahrsschrift.

Table of relative extent of solar spots each year.

Year.	Extent of spots.	Year.	Extent of spots.	Year.	Extent of spots.	Year.	Extent of spots.	Year.	Extent of spots.	Year.	Extent of spots.
1776	35·2	1792	47·5	1808	7·2	1824	6·7	1840	51·8	1856	4·2
1777	63·0	1793	40·2	1809	3·4	1825	17·4	1841	29·7	1857	21·6
1778	94·8	1794	34·3	1810	0·0	1826	29·4	1842	19·5	1858	50·9
1779	90·2	1795	22·3	1811	1·2	1827	39·9	1843	8·6	1859	96·4
1780	72·6	1796	15·1	1812	5·4	1828	52·5	1844	13·0	1860	98·6
1781	67·7	1797	7·8	1813	13·7	1829	53·5	1845	33·0	1861	77·4
1782	33·2	1798	4·4	1814	20·0	1830	59·1	1846	47·0	1862	59·4
1783	22·5	1799	10·2	1815	35·0	1831	38·8	1847	79·4	1863	44·4
1784	5·0	1800	18·5	1816	45·5	1832	22·5	1848	100·4	1864	47·1
1785	21·2	1801	38·6	1817	43·5	1833	7·5	1849	95·6	1865	32·5
1786	68·6	1802	57·8	1818	34·1	1834	11·4	1850	64·5	1866	17·5
1787	104·8	1803	65·0	1819	22·5	1835	45·5	1851	61·9	1867	8·0
1788	107·8	1804	75·0	1820	8·9	1836	96·7	1852	52·2	1868	40·2
1789	110·7	1805	50·0	1821	4·3	1837	111·0	1853	37·7	1869	84·1
1790	84·4	1806	25·0	1822	2·9	1838	82·6	1854	19·2	1870	139·6
1791	53·4	1807	15·0	1823	1·3	1839	68·5	1855	6·9	1871	109·6

These numbers have all been projected upon the lower portion of the accompanying chart, Plate II, and the curve line thus obtained is therefore to be regarded as indicating the fluctuations in the sun's spotted surface since 1776, according to the results of Dr. Wolf.

For the daily range of the magnetic declination, I have not been able to obtain any new observations with the exception of the last few years. The following are the values of the mean diurnal inequality of the magnetic declination as observed at Prague, since 1851, derived partly from the Vierteljahrsschrift and partly from the Beobachtungen zu Prag. Some of these numbers differ from those given in my former article (this Jour., vol. 50, p. 161), those values for some of the years being simply the difference between the mean declination for the year at 8 A. M. and 2 P. M.

Diurnal inequality of the magnetic declination at Prague.

Year.	Dec'n.	Authority.	Year.	Dec'n.	Authority.
1851	8′·32	Vierteljahrsschrift, iv, p.225	1862	8′·59	Vierteljahrsschrift, ix, p.116
1852	8·09	“ “	1863	8·84	“ “
1853	7·09	“ “	1864	8·02	“ x, p. 155
1854	6·81	“ “	1865	8·14	Prag Beobach., 1870, p. 16
1855	6·41	“ “	1866	7·65	“ “
1856	5·98	“ “	1867	7·09	“ “
1857	6·95	“ “	1868	8·15	“ “
1858	7·41	“ “	1869	9·44	“ “
1859	10·37	“ vi, p. 418	1870	11·41	“ 1871, p. 21
1860	10·05	“ “	1871	11·60	“ “
1861	9·17	“ vii, p. 230			

These numbers, combined with those given in my former article from 1777 to 1850 (this Jour., vol. 50, p. 161), are projected upon the accompanying chart, and the curve thence resulting is regarded as representing the mean daily range of the magnetic declination at Prague, as nearly as can be deduced from all the observations. The irregularity exhibited in my former article for the year 1791 has been corrected, as having no sufficient basis, it having resulted from the fact that, for 1790, the value employed was deduced from combining a large value at London with a small one at Mannheim, while for 1792, the value employed rested solely upon the London observation.

For the number of auroral exhibitions each year, I have depended almost exclusively upon the catalogues of auroras by Prof. Joseph Lovering. These catalogues consist of a general list of auroral displays from 500 B. C. to 1864, embracing about 10,000 cases; also a second list, embracing nearly two thousand additional cases; and a third supplementary list, containing several hundred cases not included in either of the preceding lists. Finally, there are four pages of addenda and errata, some of them of great importance. All of these materials I have endeavored to combine in a single catalogue.

In attempting to decide whether auroral displays exhibit a true periodicity, it is evident that some discrimination should be used in selecting our data for comparison. If for each year we employ the total number of auroral observations reported from all parts of the world, the numbers thus resulting exhibit a great inequality on those years for which we have reports from high northern latitudes, as happens in the years 1820, 1838, 1850, 1852, etc. Inasmuch as the observations from these high latitudes are not continuous but are confined to single years generally separated by a considerable interval, it seems necessary to exclude them entirely from the present comparison. Moreover in my former article (this Jour., vol. 50, p. 165), I have given reasons for thinking that in the high northern latitudes the inequality of the auroral displays on different years consists more in unequal *brilliancy* than in unequal *frequency* of exhibition; and therefore I have decided to leave out of the account not only those occasional observations from very high latitudes, but observations from certain lower latitudes from which the reports are tolerably complete and continuous. The geographical line of division has not, however, been drawn in an irregular manner for the purpose of including stations from which the observations would favor a pre-conceived theory, and excluding stations from which the observations were unsatisfactory, but it is designed to be a line of *equal auroral frequency*, as determined in an article published in this Journal in July, 1860. I have chosen for my northern boundary the northern line of the State of Massachusetts, and through this boundary have traced a line of equal auroral frequency across the Atlantic ocean and the continent of Europe. (See Plate I).

As the observations from the southern hemisphere do not form a long series, I exclude them entirely from the present comparison; and for the same reason I exclude the continent of Asia, and also all the western portion of the United States. I have selected as the eastern boundary the meridian of 40 degrees of longitude east from Greenwich; and as the western boundary the meridian of 80 degrees of longitude west from Greenwich. The portion of the earth's surface selected for comparison is shown on the accompanying chart, and the northern boundary of this area is shown by the undulating line crossing the chart from east to west. This line passes a little north of St. Petersburg; a little south of Abo, Upsal and Stockholm; north of Copenhagen; follows nearly the boundary between England and Scotland; passes south of Nova Scotia; follows the northern boundary of Massachusetts; and divides the State of New York by the parallel of 42° 45′. It will readily be seen that the area thus indicated embraces the whole of the earth's surface from which we have any long continued series of auroral observations, with the exception of a few stations on the north. I have endeavored to determine whether within these geographical limits a periodicity can be detected in the frequency of auroral displays, and for this purpose I have employed every known auroral observation within these limits since the year 1776, and I commence the comparison at this point because this is the date of commencement of the magnetic observations, with which the auroral observations are to be compared. The following list is supposed to contain the date of every aurora since 1776, mentioned in either of Prof. Lovering's catalogues, from any station within the geographical limits above stated. Prof. Lovering's catalogue closes with the year 1868, and as a very important maximum has recently occurred, I have endeavored to render the list as complete as possible down to the close of the year 1872. For the American observations during this period I am indebted to the kindness of Prof. Joseph Henry, Secretary of the Smithsonian Institution, who has afforded me the opportunity of examining the manuscript records of that institution. At the time of my examination, the meteorological reports for December, 1872, had not been received at Washington. To supply this omission I addressed letters to a large number of the Smithsonian observers who have been most assiduous in watching for auroras, but they all reported no aurora seen in December, 1872.

To my numerous correspondents who have kindly furnished me materials for this article, I here publicly present my acknowledgements.

For the European observations since 1867, I have depended mainly upon Heis' Wochenschrift, and have aimed to include every aurora reported in that journal from any station within the geographical limits above mentioned. A few additional cases have been derived from other journals.

Although Prof. Lovering's catalogues are very extensive, it is evident that they are not complete. On page 187 there is given a tabular statement of the number of auroras seen at St. Petersburg from 1841 to 1861, and this number is about four times as great as the number of auroras at St. Petersburg, for which the dates are given in either of the catalogues.

I have consulted all the works which were accessible to me, and in which I might expect to find a record of recent auroral observations, and have found the following sixteen cases not named in Prof. Lovering's catalogues:

1859. Feb. 28 Middletown, Conn., Prof. John Johnston.
1859. April 30. " " " " "
1859. Oct. 17. Greenwich Met. Obs., 1859, p. 161; Oxford Met. Obs., p. 30.

1859. Nov. 2. Switzerland, Comptes Rendus, vol. xlix, p. 662.
1860. March 16. Middletown, Conn., Prof. John Johnston.
1860. May 9 Oxford Met. Obs., 1860, p. 76.
1861. Jan. 20. Dorpat. Nederlandsch Met. Jaarboek, 1861, p. 286.
1861. Feb. 28. Middletown, Conn., Prof. John Johnston.
1861. March 1. " " " " " Oxford Met. Obs., p. 27.
1861. March 8. " " " " "
1861. March 10. Paris, Comptes Rendus, vol. lii, p. 465.
1861. Aug. 12. Yverdon, Switzerland, Wolf's Vierteljahrsschrift, vol. xi, p. 467.
1861. Oct. 6. Middletown, Conn., Prof. John Johnston.
1861. Oct. 12. Greenwich Met. Obs., 1861, p. 147.
1861. Oct. 26. Washington Met. Obs., 1861, p. 497.
1861. Nov. 24. Oxford Met. Obs., p. 30.

The following table shows the dates of the entire series of auroras within the geographical limits before indicated:

Catalogue of Auroras from 1776 *to* 1872.

1776. Jan. 18, 20, 21, Feb. 11, March 9, 13, 23, 28, April 8, 10, 18, 19, May 3, 21, 23, 25, June 6, 7, July 7, Aug. 14, Sept. 3, 4, 5, 6, 8, 9, 12, 16, 19, 22, 23, 24, 25, Oct. 3, 6, 7, 27, Nov. 16, Dec. 16; total 39.

1777. Jan. 13, 28, 30, Feb. 5, 6, 7, 10, 17, 26, 27, March 1, 5, 6, 9, 10, 11, 12, 14, 22, 28, 29, 31, April 1, 4. 5, 6, 7, 8, 9, 11, 12, May 4, 5, 13, 21, 30. 31, June 28, July 27, Aug. 6, 17, 24, 26, 27, 30, Sept. 4, 5, 6. 7, 15, 24, 26, 28, 30, Oct. 3, 8, 10. 13, 22, 24, 25, Nov. 3, 6, 7, 9, 21, 23, 27, 28, Dec. 1, 2, 3, 4, 5, 6, 17, 18, 21, 27, 30, 31; total 81.

1778. Jan. 18. 19, 20, 21, 25, 26, 27, Feb. 1, 15, 16 17, 18, 25, 26, 28, March 10, 15, 16, 17, 18, 19, 22, 24, 25, 26, 27. 31, April 10, 14, 15, 17, 18, 19, 20, 21, 23. 26, May 13, 14, June 10, 11, 14, 26. 28, July 3, 7, 15, 31, Aug. 12, 18, 22, 28, Sept. 1, 3, 11, 12, 15, 17, 18, 19, 20. 21, 22, 23, 24, 25, 27, 30, Oct. 9, 14, 15, 19, 23, 25, 26, 27, Nov. 20, 24, Dec. 3, 6, 7, 8, 10, 13, 14, 15, 17, 26; total 88.

1779. Jan. 6, 9, 10, 12, 13, 14, 19, Feb, 4, 6, 7, 9, 10, 11, 12, 13. 14, 15, 16, 18, 19, 25, March 2, 5, 13, 14, 15, 24, 25, 30, 31, April 2, 3, 4, 5, 6, 7, 8, 9, 10, 11, 13. 14, 17, 18. 20, 21, 22, 28, May 2, 4, 6, 7, 8, 10, 11, 12, 13, 15, 17, 20, 22, 24, 25, June 3, 5, 14, 15, 20, 24, July 9, 15, 17, 20, Aug. 3, 4, 8, 14, 17, 20, 26, 28, 29, 30, Sept. 2, 4, 8, 10, 11, 14, 15, 17, 18, 19, 22, 24, 28, 29, Oct. 3, 4, 9, 12, 14, 15, 17, 19, 22, 28. Nov. 3, 7, 8, 9, 12, 13, 14, 15, 18, 25, Dec. 3, 5, 6. 8, 9, 10; total 123.

1780. Feb. 5, 11, 15, 22, 29, March 1, 2, 29, 30, 31, April 4, 6, 13, May 8, 17, June 15, 23, 24, July 9, 11, 18, 20, 27, 28, 29, Aug. 2, 15, 27, Sept. 2, 3, 4, 7, 10, 11, 21, 22, 27, Oct. 4, 6, 10, 24, 28, 30. Nov. 4, 14, 19, 20, 21, 22, 23, 25, 26, 27, 28, 29, Dec. 7, 15, 19, 25, 27, 30, 31; total 62.

1781. Jan. 1, 17, 21, 22, 23, 25, 28, 29, 30, 31, Feb. 2, 5, 15, 16, 21, March 14, 16, 19, 20, 22, 23, 27, 28, 29, 31, April 4, 13, 14, 15, 16, 17, 19, 21, 24, 25, 26, 28, May 4, 11, 14, 16, 17, 18, June 6, 7, 8, 9, 19, 20, 28, 29, July 9, 22, Aug. 6, 8, 12. 14, 16, 17, 20, 21, 22, 23, 25. 26, Sept. 3, 7, 8, 9, 18, 19, 22, 23, 24, 25, 26, Oct. 3, 4, 8, 14, 15, 16, 19, 21, Nov. 13, 14, 15, 16, 19, Dec. 9, 10, 11, 12, 16, 19, 22, 30; total 97.

1782. Jan. 2, 4. 5, 8, 9, 10, 12, Feb, 3, 6, 15, 16, 17, 18, 25, March 7, 9, 10, 14, 15, 17, 19, 29, 30, 31, April 1, 2, 4, 6, 7, 13, 14, 27, May 4, 5, 6, 7, 9, 16, 17, 18, 20, 22, 23, 24, June 1, 4, 5, 6, 13, July 9, 10, 20, 31, Aug. 4, 5, 12, 22, 26, 27, 28, 30, Sept. 2, 3, 5, 6, 9, 10, 12, 13, 14, 15, 22, 29, 30, Oct. 1, 2, 3, 5, 8. 9, 10, 11, 14, 17, 26, Nov. 19, 20, 26, Dec. 29, 31; total 90.

1783. Jan. 9, 13, 26, 30, 31, Feb. 1, 2, 12, 21, 27, 28, March 1, 2, 4, 9, 16, 18, 20, 21, 22, 24, 25, 26, 27, 29, 30, 31, April 2, 3. 7, 9, 11, 12, 14, 18, 21, 22, 24, 25, 26. 27, 28, 29, 30, May 1, 2, 3, 4, 5, 12, 13, 16, 17, 21, 22, 29, June 1, 3, 13, 29, July 28, 30, Aug. 1, 2, 8, 9, 16, 19, Sept. 15, 25, 26, Oct. 22, 23, 24, 26, 27, 29, 30, 31, Nov. 3, 11, 13, 14, 15, 26, Dec. 4, 17, 18; total 88.

1784. Jan. 10, 11, 20, 21, Feb. 22, March 5, 12, 13, 14, 15, 18, 29, April 8, 9, 15, 16, 18, 21, May 6, 9, 12, 17, 22, June 11, 16, 17, 21, 30, July 1, 3, 24, 25, 26, Aug. 4, 5, 12, 15. 21, 30, Sept. 6. 8, 11, 12, 15, Oct. 4, Nov. 13, 15, Dec. 6, 11, 29, 31: total 51.

1785. Jan. 17, 26, 28, 29, Feb. 3, 7, 16, 17, 22, 23, March 3, 6, 19, 22, April 3, 7, 12, 26. 27, 29, May 1. 9, 15, June 5, 14, 28, July 3, 4, 17, 24, Aug. 3, 9. 11, 30, 31, Sept. 8, 11, 12, 13, 14, 23, Oct. 4, 5, 6, 8, 9, 22, Nov. 2, 5, 6, 29, 30, Dec. 13, 19, 30; total 55.

1786. Jan. 4, 14, 18, 29, 30. Feb. 3. 16, 17, 20, 27, 28, March 1, 2, 3, 4, 11, 13, 16, 18, 19, 20, 21, 22, 23, 25, 27, 28, 29, 30, 31, April 1, 2, 3, 6, 11, 12, 18, 19, 20, 21, 22, 23, 24, 25, 26, 27, 28, 29, May 1, 2, 3. 4, 5. 8, 11, 12, 14, 15, 16, 17, 18, 19, 20, 22, 23, 24, 30, 31, June 1, 4, 5, 19, 28, 30, July 1, 2, 5, 13, 15, 16, 17, 18, 19, 20, 22, 24, 25, 27, 29, Aug. 2, 3, 11, 16, 17, 18, 19, 21, 22, 24, 25, 31, Sept. 2, 3, 5, 8, 14, 17, 19, 20, 21, 26, 28, 29, Oct. 1, 2, 10, 12, 13, 14, 16, 17, 18, 19, 20, 22, 23, 25, 27, 28, Nov. 8, 12, 13, 14, 15, 16, 18, 20, 21, Dec. 3, 5, 15, 18, 20; total 143.

1787. Jan. 9, 12, 16, 17, 21, 22, 24, 25, 29, Feb. 3, 4, 6, 9, 12, 15, 17, 20, 22, 25, 27, March 6, 7, 10, 13, 16, 17, 18, 20, 21, 23, 24, 26, 31. April 2, 6, 12, 14, 16, 17, 18, 19, 20, 21, 22, 23, 26, May 4, 10, 11, 12, 13, 14, 15, 16, 17, 18, 19, 21, 25, 29, June 2, 5, 6, 7, 8, 9, 10, 14, 15, 16, 18. 19, 24, July 1, 4, 11, 13, 14, 16, 18, 20, 22, 23, 30, Aug. 1, 7, 10, 13, 19, 21, 25, 30, Sept. 6, 7, 8, 10, 18, 19, 23, 26, 28, Oct. 3, 4, 5, 6, 7, 10, 12, 13. 14. 16, 17, 19, 20, 21, 24, 31, Nov. 2, 3, 4, 7, 8, 9, 10, 12, 13, 20, 21, 24, 26. 29, 30, Dec. 1, 6, 8, 9, 10, 15, 16; total 139.

1788. Jan. 3, 4, 5, 8. 9, 10, 13, 14, 15, Feb. 2. 4, 5, 6, 7, 9, 11, 12, 15, 22. March 3, 7, 8, 14, 22, 27, 28, April 1, 2, 3, 4. 6, 7, 10. 14, 21, 24. 26, 27, 28, 29, 30, May 1. 2, 3. 4, 5, 6, 7, 10, 11, 24, 25, 26. 27, 28, 29, 30, 31, June 3. 4, 5, 9, 25, 27, 28, July 1, 2, 3, 5, 14, 15, 25, 30. 31, Aug. 1, 2, 3, 11, 19, 20, 22, 23, 24, 26, 27, 28, 29, 31, Sept. 1, 2, 3, 4, 5, 6. 7. 10, 12, 20, 24. 25, 26, 27, 28, 29, Oct. 4, 5, 6, 12, 13, 18, 21. 22, 25. 26, 27, 28, 29, 30. 31, Nov. 1, 2, 3, 18, 19. 23, 26, 27, 28, Dec. 8, 15, 16, 20, 21, 23, 30; total 135.

1789. Jan. 19, 21, 24, 29, 30, Feb. 2. 6, 15, 16, 23, 26. March 14, 20, 22, 26, 27, 28, 29. 30, 31, April 2, 9, 17, 20, 26, 27, 29, 30, May 15, 16. 17, 20, 21. 22, 23, 24, 28. 29, 31. June 2, 12, 13, 15. 16. 17, 18, 19, 20, 23, 29. 30, July 10, 11, 14, 15, 17, 18, 19. 20, 23, 24, 25, 26. 27. 28, 29, Aug. 13, 14, 15, 16, 17, 18, 19, 20, 24, 25, Sept. 9, 15. 16, 17, 20. 21, 22, 23, 24, 25, 26. Oct. 11, 16, 18, 19, 20. 21. 23, 24, 27, Nov. 4. 6, 10, 14, 15, 18, 19, 20, 21, 22, 24, 30, Dec. 10, 13, 14, 15, 16, 18, 19. 20. 22, 24: total 118.

1790. Jan. 9, 10, 18, 28, 29, Feb. 1, 3, 4, 11. 12. 13, 14, 15, 24, March 3. 8. 16, 17, 18, April 2. 3. 4, 5. 6, 7, 8. 9. 11. 16, 17, 30, May 1, 12, 14, 16. 17, 18 24, 30. June 3, 4, 5, 8, 22, 25, 30. July 1. 3, 13, 16, Aug. 3, 16, 19. Sept. 6, 7, 9, 16, 19, 24. 30, Oct. 1, 4, 9, 14, 18, 31, Nov. 4, 5, 6, 7, 8, 9, 10, 12, 16, 26. 27, 28. 30. Dec. 25. 28, 30; total 82.

1791. Jan. 6, 7. 9, 12. 20, 25, 28, 29, Feb. 21, 24, 25, 27, March 2, 3, 5, 7. 20. 22, 26. 29. April 1, 3, 5, 13. 18, 20, 23, 25, May 5, 12, 20. 23. 26, June 10, 19. 26, July 7, 20, 22, 23, 24, 28. Aug. 8, 18, 20, Sept. 8, 11, 13, 15, 27. 28, Oct. 2. 6, 15, 19, 20, 21. 22, 23, 28, 29, 31, Nov. 3, 4. 5, 8, 11, 14, 16, 17, 18, 19, Dec. 13, 19, 23, 26, 27, 28, 29; total 79.

1792. Jan. 9, 16, 17, 18, 24, 25, 30, Feb. 9, 17, March 2, 5. 15. 16, 19, 25, 27, 31, April 10, 11, 12, 13, 14. 16, May 6, 8, 9, 14, 15, 20. 22, 27, June 2. 30, July 7. 11, 18, 19, Aug. 2. 4. 17, 23. 27, Sept. 1, 6. 14, 22, 28. Oct. 12, 13, 14, 15. 16, 17, 18, 23, 31, Nov. 13, 14, 19, 29, Dec. 2, 7, 12, 13, 15, 30; total 66.

1793. Jan. 11, 12, 13, 14, Feb. 8, 12, 15, March 4, 5, 6, 13, 30, April 5, 9. 14, June 9, Aug. 6, 26, 28, Sept. 2, 4, Oct. 20, Nov. 8; total 23.

1794. Jan. 7. 22, March 8, 29, April 2, 3, 30, Nov. 11, Dec. 7. 8, 19; total 11.

1795. March 11, May 24. Sept. 8, 14, Oct. 3, 14, 16. 17, 18; total 9.

1796. Feb. 6, April 6, May 22: total 3.

1797. Jan. 22. Feb. 1, 10. 18, 27, 28, March 1, 2, 10, April 24, Nov. 18, 21, 22, 23, Dec. 20; total 15.

1798. Jan. 15; total 1.

1799. Jan. 22, Feb. 25, July 23, 24, Sept. 3, 4, Oct. 25; total 7.

1800. March 18. Aug. 15, 18, Nov. 2, 7, Dec. 10; total 6.

1801. Jan. 4, 25, Feb. 22, Aug. 18, Oct. 6; total 5.

1802. Feb. 3, March 29, June 3, 16, July 1, Sept. 19, 27, Dec. 13; total 8.

1803. Jan. 13, March 19, April 12, 13, Aug. 23, Sept. 11, 17, 19, Oct. 12, Dec. 3; total 10.

1804. Feb. 2, March 29, April 1, 4, May 2, 12, Sept. 7, Oct. 12, 22, Nov. 5, 22, 25; total 12.

1805. Jan. 1, 4, Feb. 23, March 26, April 30, May 27, 28, Aug. 29, Sept. 15, 21, 22, 24, Oct. 13, 20, 22, Nov. 16, 18, 19, 20, 25, 26, Dec. 26; total 22.

1806. Jan. 11, Feb. 7, March 16, April 13, Aug. 9, Sept. 10, Oct. 2, 5, Nov. 2, 30, Dec. 22; total 11.

1807. Jan. 13, 26, March 26, April 11, May 8; total 5.

1808. March 8, July 23, 31; total 3.

1809. Jan. 31, June 13; total 2.

1810. Oct. 5; total 1.

1811. None.

1812. None.

1813. June 24, Sept. 24; total 2.

1814. Feb. 28, April 7, 14, 15, 17, May 22, Sept. 10, 11, Oct. 22; total 9.

1815. March 2, May 29, Sept. 26; total 3.

1816. May 17, Sept. 15, Oct. 7; total 3.

1817. Feb. 6, 8, 9, 10, 18, 20, March 4, June 12, Aug. 16, Sept. 19, Oct. 17; total 11.

1818. Jan. 11, April 4, May 23, 28, June 6, 7, 8, 9, 10, Sept. 20, 24, 25, 26, 27, Oct. 6, 7, 26, 31; total 18.

1819. Feb. 1, 19, March 25, 26, April 26, Oct. 3, 12, 15, 17, 31, Nov. 13, 14, Dec. 14; total 13.

1820. Jan. 14, Feb. 11, April 3, May 1, Dec. 4; total 5.

1821. March 25, Nov. 26, Dec. 12; total 3.

1822. Feb. 13, Oct. 22; total 2.

1823. None.

1824. Aug. 10, Sept. 29; total 2.

1825. March 19, April 14, Aug. 17, Sept. 11, Oct. 7, Nov, 3, 22, Dec. 7; total 8.

1826. Jan. 16, 21, March 29. April 29, Sept. 9, Oct. 2; total 6.

1827. Jan. 9, 16, 18, Feb. 13, 17. April 6. 16, Aug. 25, 26, 27, 28, 29, 31, Sept. 8, 9, 25, 26, 27, 28, Oct. 6, 17, 19, Nov. 9, 18, 19, Dec. 8, 27; total 27.

1828. Jan. 16, 18, 19, Feb. 12, April 11, 12, June 2, July 5, Aug. 14, Sept. 8, 15, 25, 26, 27, 29, 30, Oct. 3. 8, 9, 11, 15, 29. Dec. 1, 22, 26, 28; total 26.

1829. Jan. 2, 28, Feb. 11, March 4, 6. 18, 22, 23, April 4, May 2, 4, June 17, 21, 25. July 25, Aug. 25, 29, Sept. 18, 19, 21, 26, Oct. 3, 6, 11, 17, 25, 27, Nov. 17, 18. 19, Dec. 14, 19, 20, 28; total 34.

1830. Jan. 25, 28, Feb. 19, 23, 24, March 15. 18, 24, 28. April 15, 19, 20, 24, May 4, 5, 6, 9, 10, 13, 14, 15, 19, June 10, 11, 17, 29, July 14, 15, 28, Aug. 15, 20, 28, Sept. 7. 8, 9, 10, 12, 13, 15, 16, 17, 18, 19, 20, 21, 23, 25, Oct. 5, 6, 7, 8, 9, 10, 13. 16, 17, 18, 22, 28, Nov. 1, 3, 4, 7, 10, 19, 20, Dec. 7, 8, 10, 11, 12, 13, 14, 15, 22, 25, 30; total 77.

1831. Jan. 5, 6, 7, 8, 10, 11, 21, Feb. 6, 7, 11. 14, 17, March 1, 2, 3, 6, 7, 8, 9, 10, 11, 12, 13, 15, 24, April 1, 3, 18, 19, 20, 23, May 8, 30, June 10. 11, 16, 17. 21, July 3, 4, 5, 10, 30, 31, Aug. 3, 4. 5, 19, Sept. 12, 13, Oct. 29, Nov. 29; total 52.

1832. Jan. 22, 27, Feb. 2, March 27 April 10. May 12. June 27, Aug. 22, 23, 24, Sept. 17, 23, 30, Oct. 7, 12, Nov. 1, 4, 12. 13, 14, 27, Dec. 21; total 22.

1833. Jan. 2, Feb. 11, 18, 19, March 13, 21, April 24, May 16. 17, 18, June 14, 17, July 10, 13. Aug. 1, 6, 21, Sept. 1, 2. 5, 10, 12, 17, 18, Oct. 4, 12. 13, 14, Nov. 3, 12, 13, Dec. 15, 29, 30; total 34.

1834. Jan. 5, 7, 15, Feb. 7, 8, 10, 20, March 3. 4. May 3, June 28, July 28, Sept. 30, Oct. 1, 8, 23, Nov. 2. 3, 5, 6, 28, Dec. 3 4, 6, 21, 22, 23; total 27.

1835. Jan. 4, 29, Feb. 7. March 1, June 21, July 29, Aug. 19, Sept. 4, 7, 9, 22, 24, Oct. 18, Nov. 17, 18, Dec. 10, 21; total 17.

1836. Feb. 11, March 17, April 20, 21, 22. 23, May 8, 19, 20, June 5, 8, 9, 10, July 3, 30, 31, Aug 1, 2. 10, 11, 12, 13, 14, 21, Sept. 13, 15, 29, Oct. 10, 11, 12, 15, 18, 19, Nov. 1, 15, 27; total 36.

1837. Jan. 2, 24, 25, 26, Feb. 13, 14, 18, March 1, 10, 29, April 6, 21, May 2, 6, 19, June 1, 2, 3, 24, July 1, 2, 3, 7, 25, 28, 29, Aug. 3, 25, 27, 28, 29, Sept. 18, 20, 21, 22, 23, 24, 29, Oct. 6, 18, 21, 22, Nov. 1, 5, 6, 12, 14, 15, 17, 18, 30, Dec. 1, 4, 5, 28, 30; total 56.

1838. Jan. 5, 15, 16, 24, 25, 28, Feb. 3, 4, 18, 20, 21, 22, 23, 26, March 15, 19, April 12, 29, 30. May 13, 14, June 25, 26, July 13, 14, 15, 27, 29, Aug. 22, 23, 28, Sept. 5, 7, 10, 13, 14, 15, 16, 17, 20, Oct. 16, Nov. 12, 13, 14, 25, 26, Dec. 13; total 47.

1839. Jan. 10, 11, 14, 16, 19. 21, Feb. 4, 18, March 5, 10, 15, 19, 22, 24, April 7, 8, 20, 21. May 5, 7, 9, 10, 11, 14, 15, 17, 31, June 6, 7, 12, July 3, 4, 30, Aug. 10, 20, 28, 31, Sept. 1, 2, 3, 4, 14, 15, 19, 21, 28, Oct. 8, 10, 11, 22, 31, Nov. 4, 6, 8, 11, 13, 17, 23, 30, Dec. 4, 14, 21, 31; total 63.

1840. Jan. 3, 4, 5, 8, 30, 31, Feb. 6, 7, 21, March 5, 6, 22, 29, 31, April 1, 4, 5, 15, 19, 20, 23. 24, May 1, 6, 20, 28, 29, 30, June 1. 10, 14, 21, 22, 26, July 4, 16, 25, 29, Aug. 19, 24, 25, 26, 28, 29, 31, Sept. 1, 19, 21, 22, 23. Oct. 21, 22, 23, 27, 29, 31, Nov. 12, 13, 15, 19, 20, 23, 29, 30, Dec. 11, 13, 14, 17, 19, 21, 22, 24, 28, 29; total 74.

1841. Jan. 14. 22, 25, 27, 28, Feb. 7, 8, 11, 12, 15, 18, 19, 22, 23, 24, March 11. 12, 14, 15, 16, 17, 19. 20, 21, 22, 23, 24, 25, April 11, 14, 16, 18, 19, May 3, 8, June 11, 15, 16, 17, 21, July 11, 19, 21, 24, 29, Aug. 2, 5, 6, 14, 23, Sept. 10, 12, 13, 14, 18, 25, 26, Oct. 6, 9, 13, 14, 15, 18, 19, 20, 21, 25, Nov. 5, 8, 15, 17, 18, Dec. 1, 12, 13, 14, 15, 19, 20, 21, 24; total 81.

1842. Jan. 2, 4, 5, 7, 9, 11, 12, 15, 20, 24, Feb. 1, 2, 6, 11, 12, 13, 24, March 3, 7, 15, 23, April 3, 10, 11, 12, 13, 14, 15, 16, 20, June 7, 9, 30, July 3, 11, 31, Aug. 5, 8, 9, 23, Sept. 2, 28, Oct. 7, 16, 17, 18, Nov. 3, 6, 21, 24, 28, Dec. 17, 27; total 53.

1843. Jan. 2, Feb. 24, March 2, 4, 6. 12, 13, 17, April 2, 5, 6, 22, May 6, 7, 9, 13, 17, 23, June 15, 27, 30, July 7, 19, 25, Aug. 3, Sept. 13, 18, 27, 28, Oct. 14, 19, Nov. 15, 27, Dec. 8, 12, 29; total 36.

1844. Jan. 8, 12, 13, 19, 24, 27, Feb. 11, 20, March 4, 7, 25, April 5, 17, May 7, 8, 14, 22, June 12, 16, 22, Aug. 1, 9, 11, 29, Sept. 30, Oct. 20, Nov. 5, 6, 13, 14, 16, Dec. 8, 14, 29; total 34.

1845. Jan. 6, 9, 10, 12, 29, Feb. 13, 25, March 9, 16, 18, April 13, 27, 30, May 1, 5, 29, June 5, 17, 24, 25, July 4, 8, 24, 25, 28, 31, Aug. 1, 4, 29, 30, Sept. 1, 7, 24, 25, 26, 27, Oct. 9, 21, Nov. 27, Dec. 2, 3, 4, 5, 14, 16, 29, 30, 31; total 48.

1846. Jan. 3, 4, 19, 20, 23, 28, 31, Feb. 18, 19, 25, 26, March 14, 17, 25, 28, April 6, 12, 15, 16, May 3, 4, 10, 12, 13, 14, 18, 19, June 14, July 12, 14, 15, 25, Aug. 6, 11, 12, 18, 24, 28, 29, 30, Sept. 1, 9, 10, 11, 21, 22, 23, Oct. 7, 8, 10, 11, 15, 19, 21, 22, 24, Nov. 11, 13, 17, 18, 21, Dec. 4, 9, 11, 13, 14, 16, 22, 23; total 69.

1847. Jan. 13, 14, 17, 21, 25, Feb. 6, 7, March 5, 8, 11, 17, 19, 26, 28, April 4, 6, 7, 16, May 7, 15, June 6, 12, July 9, 11, Aug. 3, 4, 21, 25, Sept. 13, 15, 16, 24, 26, 28, 29, 30, Oct. 3, 13, 14, 24, 25, 27, Nov. 1, 2, 10, 11, 12, 14, 18, 25, 26, Dec. 4, 15, 16, 17, 19, 20, 31; total 58.

1848. Jan. 3, 6, 16, 23, 28, Feb. 4, 6, 7, 8, 15, 18, 20, 21, 22, 23, 24, 25, 26, March 7, 8, 15, 17, 19, 20, 22, 24, 25, 28, 29, 30, 31, April 1, 2, 3, 5, 6, 7, 15, 17, 29, May 13, 18, 24, 25, 27, 31, June 1, 22, July 3, 4, 7, 8, 11, 23, 24, Aug. 8, 14, 19, 21, 22, Sept. 3, 4, 5, 9, 23, 30, Oct. 5, 6, 8, 10, 17, 18, 19, 20, 21, 22, 23. 24, 25, 26, 27, 30, Nov. 13, 14, 15, 17, 18, 19, 21, 22, 23, 24, 25, 26, 27. 28, 29, 30, Dec. 1, 13, 16, 17, 18, 22, 23, 26, 27, 29; total 108.

1849. Jan. 4, 14, 15, 22, 25, 30, Feb. 13, 16, 18, 19, 20, 21, 22, 23, 24, 26, 27, March 16, 17, 18, 19, 25, April 1, 2, 13, 14, 17, 20, 22, 24, 25, 30, May 12 13, 30, 31, June 10, 12, 15, 17, 19, 22, 24, 26, 30, July 1, 10, 11, 12, 13, 22, 23, 24, Aug. 10, 13, 16, 18, Sept. 3, 7, 9, 11, 13, 16, 18, 19, 20, 23, Oct. 1, 4, 13, 14, 15, 18, 20, 22, Nov. 9, 12, 27, 30, Dec. 7, 8, 17; total 82.

1850. Jan. 1, 5, 12, 18, 19, 27. 30, Feb. 1, 3, 4, 6, 9, 10, 12, 13, 14, 15, 21, 22, 26, March 1, 2, 3, 4, 9, 10, 11, 12, 15, 22, 25, 26, 27, 28, 29, 31, April 1, 2, 5, 6, 7, 8, 9, 16. 17, 18, 27, 29, May 3, 7, 8, 12, June 4, 5, 8, 11, 12, 13, 24, 26, 27, 29, July 3, 5, 6, 9, 10, 11, 12, 21, 22, 27, 28, Aug. 6, 9, 10, 11, 12, 13, 16, 17, 18, 21, 26, 28, 29, 30, Sept. 3, 4, 6, 8, 10, 11, 12, 13, 14, 15, 28, 29, 30, Oct. 1, 2, 3, 6, 7, 11, 14, 27, 28, 29, 30, Nov. 4, 9, 10, 12, 21, 28, Dec. 1, 3, 20, 26, 27; total 122.

1851. Jan. 1, 5, 9, 12, 19, 21, 23, 24, Feb. 1, 5, 6, 7, 11, 12, 17, 18, 19, 20, 23, 24, 26, 28, March 3, 6, 7, 8, 10, 14, 18, 20, 21, 22, 23, 24, 25, 28, 29, 30, April 3, 7, 8, 10, 18, 21, 24, May 1, 4, 5, 15, 16, 17, 24, 26, June 5, 6, 11, 19, 20, 23, 24, 27, July 7, 17, 26, Aug. 13, 14, 21, 24, 29, 30, Sept. 3, 4, 6, 7, 10, 15, 16, 26, 27, 28, 29, 30, Oct. 1, 2, 3, 14, 15, 18, 20, 21, 23, 28, 29, Nov. 4, 5, 13, 15, 20, 21, 24, 26, Dec. 6, 8, 22, 23, 26, 28, 29, 30; total 109.

1852. Jan. 4, 17, 19, 20, 21, 23, 25, 30, 31, Feb. 4, 15, 16, 17, 18, 19, 20, 21, 23, 25, 26, 27, 28, March 2, 4, 5, 7, 9, 17, 21, 25, 26, 31, April 1, 9, 10, 11, 12, 15, 16, 20, 21, 22, 23, 26, May 1, 2, 3, 10, 17, 18, 19, 20, 21, 25, June 5, 10, 11, 12, 15, 16, 19, 24, July 3, 5, 6, 7, 8, 9, 10, 11, 12, 13, 14, Aug. 8, 11, 14, 15, 24, Sept. 2, 4, 5, 6, 12, 16, 17, 18, 21, 29, Oct. 5, 9, 10, 13, 18, 19, 20, 25, Nov. 3, 7, 10, 11, 12, 14, 15, 16, 30, Dec. 2, 5, 6, 8, 13, 14, 15, 17, 29; total 114.

1853. Jan. 4, 5, 6, 7, 8, 9, 30, 31, Feb. 1, 14, 15, 16, 17, 27, 28, March 7, 8, 9, 10, 13, 14, 17, 18, 27, 29, April 5, 6, 7, 8, 9, 10, 11, May 2, 4, 5, 7, 13, 14, 24, 31, June 1, 6, 9, 14, 22, July 12, 16, Aug. 11, 20, 30, Sept. 1, 2, 3, 4, 28, Oct. 6, 23, 25, 29, 30, 31, Nov. 1, 2, 8, 22, Dec. 5, 6, 23, 24, 26, 27, 28, 29, 30; total 74.

1854. Jan. 2, 23, 28, Feb. 2, 4, 16, 17, 27, March 15, 16, 21, 27, 28, 29, 30, April 20, 21, 23, May 1, 15, 16, 19, 21, June 12, 13, July 10, 27, Sept. 13, 14, 21, 22, 23, 26, 27, Oct. 18, Nov. 20; total 36.

1855. Jan. 10, Feb. 5, 6, 12, March 12, 18, April 7, 9, 12, 14, May 10, Sept. 10, 17, Oct. 4, 8, 9, Nov. 29; total 17.

1856. March 12, 30, April 22, June 2, 4, 6, Aug. 22, 24, 31, Oct. 4, 19, 23, Nov. 20; total 13.

1857. Feb. 26, Sept. 1, 3, 7, 8, 10, Nov. 9, 17, Dec. 16, 17, 18, 20; total 12.

1858. Jan. 8, 12, Feb. 16, March 1, 12, 13, 14, April 1, 9, 10, 11, 12, 14, 15, 21, 24, 29, May 1, 2, 6, 7, 10, June 6, July 1, 5, Aug. 30, 31, Sept. 1, 7, 8, 9, 12, 14, Oct. 2, 4, 6, 7, 8, 9, 10, 27, 31, Nov. 1, 10, Dec. 1, 4, 24; total 47.

1859. Jan. 1, 22, 23, Feb. 9, 22, 23, 24, 25, 26, 28, March 2, 25, 26, 27, 29, 30, 31, April 1, 12, 21, 22, 23, 24, 28, 29, 30, May 1, 5, July 15, 28, Aug. 21, 28, 29, 30, 31, Sept. 1, 2, 3, 4, 5, 6, 24, 25, 27, 28, Oct. 1, 2, 12, 17, 18, 19, 20, 21, 24, Nov. 2, 12, Dec. 1, 6, 13, 14, 22, 24; total 62.

1860. Jan. 15, Feb. 12, 21, 22, March 12, 16, 17, 18, 19, 22, 24, 25, 26, 27, 28, April 9, 12, 13, 14, 15, 16, 18, May 6, 9, 10, 23, June 30, July 19, Aug. 6, 7, 8, 9, 10, 11, 12, 13, 14, 16, 18, 25, 27, 30, Sept. 6, 7, 8, 15, 16, 25, Oct. 1, 8, 11, 19, Nov. 2, 4, Dec. 8, 10, 15; total 57.

1861. Jan. 12, 13, 20, 22, 23, 24, 31, Feb. 2, 12, 27, 28, March 1, 2, 4, 8, 9, 10, 25, April 7, 8, 15, 25, 26, 28, 29, June 12, Aug. 4, 5, 12, Sept. 8, 10, 11, 15, 25, Oct. 2, 6, 10, 12, 24, 25, Nov. 5, 7, 24, Dec. 2, 3, 4, 19, 20, 23, 24, 26; total 51.

1862. Feb. 21, 22, March 4, 5, 14, April 2, 13, May 2,19, 20, June 4, July 5, 19, 27, Aug. 2, 4, 14, 18, 21, 28, 29, 30, Sept, 11, 24, 25, 27, 28, 30, Oct. 4, 9, 10, 11, 15, 17, 26, Nov. 29, Dec. 14, 15, 24; total 39.

1863. Jan. 25, Feb. 8, 9, 22, 23, March 18, 21, 22, April 6, 9, 19, 21, May 6, 8, 9, June 23, Sept. 7, 8, 9, 23, Oct. 7, 8, 11, Nov. 9, 10, 13, 14, 29, Dec. 10, 11; total 30.

1864. Jan. 5, 6, 14, 17, March 6, 9, 10, 14, April 5, 27, June 7, 18, Aug. 1, 2, 6, 9, 31, Sept. 13, 20, 25, 27, Oct. 8, 15, 19, 21, 29, Nov. 19, 23, 30, Dec. 11, 18, 22, 23, 24, 29, 30, 31; total 37.

1865. Jan. 9, 13. 17, 24, 25, 27, 29, 30, Feb. 7, 17, 18, 20, 21, 22, 23, 27, March 7, 17, 18, 19, 20, 21, 22, 23, 28, 29, April 16, June 17, July 12, 19, Aug. 2, 14, 18, 19, 25, 26, Sept. 12, 15, 16, 17, 21, 26, 28, Oct. 12, 13, 14, 28, 31, Nov. 9, 14, Dec. 15; total 51.

1866. Jan. 8, 16, 27, Feb. 7, 12, 13, 14, 16, March 5, 6, 7, 17, 18, 22, 29, April 16, 17, May 3, 14, June 15, July 12, Sept. 6, 12, Oct. 3, 4, Nov. 9, 11, 19, 29, Dec. 25; total 30.

1867. May 2, 4, 27, July 28, Sept. 21, 23, 25, 26, 29; total 9.

1868. March 19, 24, April 13, 17, 18, 28, May 12, July 10, Sept. 19, 21, 22, Oct. 14, 22, Nov. 14, 16, 19, Dec. 4, 12, 13, 14; total 20.

1869. Jan. 5, 6, 11, 12, 13, 17, 20, 21, Feb. 3, 4, 5, 11, 15, 19, March 1, 5, 7, 9, 10, 14, 18, 31, April 1, 2, 5, 6, 7, 8, 9, 10, 11, 15, 16, 17, May 3, 4, 5, 6, 7, 8, 9, 11, 12, 13, 24, 28, 29, June 5, 6, 7, 8, 29, 30, July 15, 16, Aug. 2, 5, 6, 10, 24, 27, Sept. 2, 3, 4, 5, 9, 10, 11, 12, 13, 14, 15, 22, 24, 25, 26, 27, 28, 29, Oct. 3, 5, 6, 17, 18, 21, 25, 31, Nov. 3, 10, 12, 25, Dec. 7, 13, 15, 25; total 95.

1870. Jan. 1, 2, 3, 4, 5, 6, 8, 9, 16, 18, 20, 25, 26, 28, 29, 30, 31, Feb. 1, 2, 3, 4, 5, 11, 12, 13, 17, 19, 22, 23, 24, 28, March 1, 3, 4, 5, 6, 8, 9, 13, 14, 19, 21, 22, 24, 25, 28, 30, 31, April 1, 5, 15, 18, 21, 23, 24, 25, 29, May 1, 19, 20, 22, 27, 31, June 18, July 18, 19, 27, 30, Aug. 7, 12, 19, 20, 21, 28, 29, 31, Sept. 2, 3, 4, 15, 17, 18, 20, 21, 23, 24, 25, 26, 27, 28, Oct. 1, 2, 3, 12, 13, 14, 15, 17, 18, 20, 21, 22, 23, 24, 25, 26, 27, 28, 29, 30, Nov. 1, 8, 14, 16, 17, 18, 19, 21, 22, 23, 24, 25, 27, Dec. 10, 15, 16, 17, 21, 22, 23; total 130.

1871. Jan. 12, 13, 15, 16, Feb. 5, 9, 10, 11, 12, 13, 15, 18, 21, 22. 26, March 1, 2, 10, 12, 13, 14, 16, 17, 18, 19, 21, 22, 23, 24, April 1, 5, 8, 9, 10, 11, 13, 14, 15, 16, 17, 18, 19, 21, 22, 23, 24, 28, 29, May 1, 6, 7, 8, 9, 10, 16, 17, 19, 20, 22, 25, 26, June 7, 10, 11, 12, 14, 16, 17, 18, 21, 26, 27, July 14, 15, 22, Aug. 8, 10, 11, 12, 14, 16, 17, 21, 22, 26, Sept. 4, 6, 7, 8, 9, 10, 11, 13, 15, 20, Oct. 4, 9, 12, 13, 15, 16, 17, 18, 19, Nov. 1, 2, 3, 5, 6, 9, 10, 11, 14, 15, 19, 20, 21, 22, 30, Dec. 7, 8, 9, 10, 14, 28; total 125.

1872. Jan. 5, 6, 7, 10, 15, 30, 31, Feb. 2. 4, 5, 6, 8, 9, 10, 15, 17, 19, 24, 25, 26, 27, 29, March 1, 2, 4, 5, 6, 7, 8, 9, 10, 11, 12, 15, 16, 20, 26, 27. 28, 29, 30, 31, April 1, 2, 3, 5, 6, 7, 8, 9, 10, 11, 12, 13, 14, 15, 17, 18, 22, 24, 26, 27, 28, 29, 30, May 3, 6, 7, 25, 26, 27, 28, June 3, 9, 22, 30, July 1, 7, 8, 20, 26, 27, 28, Aug. 1, 3, 4, 5, 8, 9, 10, 14, 24, 25, 27, Sept. 2, 3, 4, 16, 17, 28, Oct. 2, 5, 14, 23, 24, 25, 26, 28, 29, Nov. 1, 2, 4, 8, 9, 10, 13, 19, 23, 24, 25, Dec. 3, 6; total 122.

The total number of auroras for each year as shown in the preceding list is given in column second of the following table. In order to eliminate the effects of irregular and non-periodic causes, I have taken the average of the numbers in column second for each successive period of three years, and the numbers thus resulting are given in column third.

Number of Auroras from 1776 *to* 1872.

Year	Auroras	Mean	Year	Auroras	Mean	Year.	Auroras	Mean	Year	Auroras	Mean	Year	Auroras	Mean
1776	39	..	1796	3	9	1816	3	6	1836	36	36	1856	13	14
1777	81	69	1797	15	6	1817	11	11	1837	56	46	1857	12	24
1778	88	97	1798	1	8	1818	18	14	1838	47	55	1858	47	40
1779	123	91	1799	7	5	1819	13	12	1839	63	61	1859	62	55
1780	62	94	1800	6	6	1820	5	7	1840	74	73	1860	57	57
1781	97	83	1801	5	6	1821	3	3	1841	81	69	1861	51	49
1782	90	92	1802	8	8	1822	2	2	1842	53	57	1862	39	40
1783	88	76	1803	10	10	1823	0	1	1843	36	41	1863	30	35
1784	51	65	1804	12	15	1824	2	3	1844	34	39	1864	37	39
1785	55	83	1805	22	15	1825	8	5	1845	48	50	1865	51	39
1786	143	112	1806	11	13	1826	6	14	1846	69	58	1866	30	30
1787	139	139	1807	5	6	1827	27	20	1847	58	78	1867	9	20
1788	135	131	1808	3	3	1828	26	29	1848	108	83	1868	20	41
1789	118	112	1809	2	2	1829	34	46	1849	82	104	1869	95	82
1790	82	93	1810	1	1	1830	77	54	1850	122	104	1870	130	117
1791	79	76	1811	0	0	1831	52	50	1851	109	115	1871	125	126
1792	66	56	1812	0	1	1832	22	36	1852	114	99	1872	122	....
1793	23	33	1813	2	4	1833	34	28	1853	74	75	....	....	....
1794	11	14	1814	9	5	1834	27	26	1854	36	42	....	....	....
1795	9	8	1815	3	5	1835	17	27	1855	17	22	....	...	....

The average number of auroras for each period of three years has been projected upon the accompanying chart, Plate II, which accordingly shows the fluctuations in the frequency of auroras for a period of 96 years. This auroral curve shows great irregularities in the number of auroral exhibitions, but affords unmistakable evidence of a periodic alternation of seasons of abundance with seasons of scarcity.

If we compare the curve showing the mean daily range of the magnetic declination with the curve showing the relative extent of the solar spots, we find a very close correspondence of the curves, and the coincidence in the times of maximum and minimum is quite surprising. The following table shows the dates of maximum and minimum of these two classes of phenomena estimated to the nearest half year. Column third shows the difference between the dates in the first two columns.

Date of Maximum.			Date of Minimum.		
Solar Spots.	Magnetic Declination.	S— M.	Solar Spots.	Magnetic Declination.	S— M.
1778	1777	+1·0	1784	1784	·0
1788.5	1787	+1·5	1798	1799·5	—1·5
1804	1803	+1·0	1810	wanting.	unknown.
1816.5	1817.5	—1·0	1823	1823·5	—0·5
1829.5	1829	+0·5	1833·5	wanting.	unknown.
1837	1838	—1·0	1843·5	1844	—0·5
1848.5	1848.5	·0	1856	1856	·0
1860	1859.5	+0·5	1867	1867	·0
1870	1870.5	—0·5	----	----	----

In only two instances does the difference between the dates of the critical periods of the two phenomena exceed one year. In 1787 the magnetic curve attained its maximum and the sun-spot curve also reached nearly its maximum on the same year, but this unusual solar disturbance was prolonged for a period of three years. In 1798 the sun-spot curve reached its minimum, but the magnetic curve oscillated near its minimum for a period of five years. It is noticeable that the correspondence between the dates of the critical periods for the two phenomena is closest for the more recent period, in which the observations are most numerous and most reliable, and we may therefore presume that a portion of the discrepancy in the undulations of the two curves is due to the imperfection of the earlier observations. A comparison of these observations seems to justify the following conclusions contained in my former article (this Journal, vol. 50, p. 160).

1. A diurnal inequality of the magnetic declination, amounting at Prague to about six minutes, is independent of the changes in the sun's surface from year to year.

2. The excess of the diurnal inequality above six minutes as observed at Prague is nearly proportional to the amount of spotted surface upon the sun, and may therefore be inferred to be produced by this disturbance of the sun's surface, or both disturbances may be ascribed to a common cause.

The correspondence between the auroral curve and the sun-spot curve, though not as close as between the magnetic curve and the sun-spot curve, is certainly quite remarkable. The following table shows the dates of maximum and minimum of these two classes of phenomena. Column third shows the difference between the dates in the first two columns.

Date of Maximum.			Date of Minimum.		
Solar Spots.	Auroras.	S— A.	Solar Spots.	Auroras.	S— A.
1778	1778	0	1784	1784	0
1788·5	1787·5	+1·0	1798	1798	0
1804	1804·5	—0·5	1810	1811	—1·0
1816·5	1818	—1·5	1823	1823	0
1829·5	1830	—0·5	1833·5	1834·5	—1·0
1837	1840	—3·0	1843·5	1843·5	0
1848·5	1850·5	—2·0	1856	1856	0
1860	1859·5	+0·5	1867	1867	0
1870	1870·5	—0·5	----	----	----

In only two cases is there any sensible difference in the dates of minimum of the two classes of phenomena. Also in the year 1810 only one aurora was recorded, so that this year presents no real discrepancy in the dates of minimum.

From 1832 to 1835 the number of auroras was quite small, so that for the entire series of observations we may say there is an almost complete identity in the dates of minimum of the two classes of phenomena.

With regard to the dates of maximum there is some discordance, which in 1840 amounts to three years. It is also noticeable that the magnetic curve remained nearly stationary from 1836 to 1838 while the sun-spot maximum was sharply defined, suggesting the idea that the connection between the auroral and magnetic curves is more intimate than between the auroral and sun-spot curves.

The discrepancy in 1850 is apparently due to a double or triple maximum of the auroral curve. The New Haven observations show a maximum in 1848, and another in 1852; the combination of all the observations shows three maxima, viz., in 1848, 1850 and 1852, the greatest frequency being in 1850; that is, there was a prolonged period of unusual auroral displays extending over several years.

A comparison of both maxima and minima indicates that the critical periods of the auroral curve occur a little later than those of the sun-spot curve and that the auroral maximum is frequently more prolonged than the sun-spot maximum.

If we institute a comparison between the auroral curve and the magnetic curve we shall find the correspondence to be still more remarkable. The following table shows the dates of maximum and minimum of these two classes of phenomena. Column third shows the difference between the dates in the first two columns.

Date of Maximum.			Date of Minimum.		
Magnetic Declination.	Auroras.	M−A.	Magnetic Declination.	Auroras.	M−A.
1777	1778	−1 0	1784	1784	0·0
1787	1787·5	−0·5	1799·5	1798	+1·5
1803	1804·5	−1·5	wanting.	1811	
1817·5	1818	−0·5	1823·5	1823	+0·5
1829	1830	−1·0	wanting.	1834·5	
1838	1840	−2·0	1844	1843·5	+0·5
1848·5	1850·5	−2·0	1856	1856	0·0
1859 5	1859·5	0·0	1867	1867	0·0
1870·5	1870·5	0·0	----	----	----

We perceive that the auroral maximum generally occurs a little later than the magnetic maximum, the average difference amounting to one year; while the time of auroral minimum either coincides with the magnetic minimum or slightly precedes it, the average difference amounting to about half a year.

On the whole there seems to be no room for question that the number of auroras seen in the middle latitudes of Europe and America exhibits a true periodicity, following very closely the magnetic periods but not exactly copying them. In particular we notice that during those periods in which the range of the magnetic declination was unusually small, as from 1794 to 1824, auroral exhibitions were extremely few in number and insignificant in respect of brilliancy.

If now we inquire as to the probable connection between these three classes of phenomena, we cannot suppose that a small black spot on the sun exerts any direct influence on the earth's magnetism or electricity, but we must rather conclude that the black spot is a result of a disturbance of the sun's surface which is accompanied by an emanation of some influence from the sun, which is almost instantly felt upon the earth in an unusual disturbance of the earth's magnetism, and a flow of electricity developing the auroral light in the upper regions of the earth's atmosphere. The appearances favor the idea that this emanation consists of a direct flow of electricity from the sun. If we maintain that light and heat are the result of vibrations of a rare ether which fills all space, the analogy between these agents and electricity would lead us to conclude that this agent also is the result of vibrations in the same medium, or at least that it is a force capable of being propagated through the ether, with a velocity similar to that of light. While this influence is traveling through the void celestial spaces it develops no light, but as soon as it encounters the earth's atmosphere, which appears to extend to a height of about 500 miles, it develops light, and its movements are controlled by the earth's magnetic force, in a manner analogous to the influence of an artificial magnet upon a current of electricity circulating round it.

ART. XXVII.—*Notices of Recent Earthquakes*; by Prof. C. G. ROCKWOOD, Jr., Bowdoin College.

April 3, 1872. "A letter from Beyrout gives some statistics of the earthquake at Antioch in April last.* Before the shock there were 3,003 dwelling houses in the city. Of these 1,960 were ruined, and 894 so damaged as to be uninhabitable, leaving only 149 in good condition. These were besides 1,331 other buildings,—shops, mosques, factories, &c. Of these there are left but 349 shops, one mosque, and one soap factory—so that of the 4,334 buildings of all kinds only 500 are left. The population was about 17,600, of whom 500 were killed and an equal number wounded. In Luedia there were 2,150 houses ruined, and more than 300 persons were killed or wounded."—*Boston Advertiser*, Aug. 13, 1872.

April 24, 1872. Nearly coincident with the eruption of Vesuvius, there was a violent eruption of the volcano Merapi in Java. Streams of lava and boiling water were accompanied by showers of ashes and sand, which obscured the light of the sun, and some 150 persons are reported to have perished. The eruption was attended with slight shocks of earthquake.

June 4, 1872. A slight shock was felt between 10 and 11 P. M. at Chesterfield, Manchester and Ashland, Va., and also at Charlottesville, Va.

June 17, 1872. A sharp shock was felt about 3 P. M. at Milledgeville, Ga. Brick buildings were jarred and windows rattled.

July 8, 1872. A short but distinct shock was felt at 10½ P. M. at Chillicothe, Mo., accompanied by a rumbling noise.

July 11, 1872. A sharp shock was felt at 5.25 A. M. across the southern part of Westchester Co., N. Y. It extended from the Hudson river to and across the western end of Long Island Sound. The area affected would be pretty well covered by a circle described from New Rochelle, N. Y., as a center, with a radius of ten miles. The phenomena are thus described in a letter written from Glen Cove, L. I., to the Smithsonian Institute: "The duration of the trembling motion was about two or three seconds, and its intensity was sufficient to wake almost every one who had not yet awakened. The shock was accompanied by a rumbling noise at the beginning, lasting about one or two seconds; then a tremendous discharge of noise like a violent burst of thunder, at which time the jarring took place; then passing away with the rumbling sound like the passing off of thunder from a violent burst near by. The direction appeared to be from E.N.E. to W.S.W." Other accounts speak vaguely of it as "coming from the south and rolling away off toward the north." At Port Washington, L. I., a clock with a long pendulum was stopped at 26 minutes past 5 A. M. No damage was done anywhere.

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* This Journal, III, iv, p. 4.

August 13, 1872. The eruption of Mauna Loa, in the Sandwich Islands (described in this Journal, III, iv, pp. 331, 406), was attended with earthquake shocks in places near the volcano.

Sept. 7, 1872. Vesuvius was emitting smoke from two craters. Slight shocks were felt at the foot of the mountain.

Sept. 14, 1872. Earthquakes, accompanied by underground explosions, began again in Owen's River valley, Inyo Co., Cal.

Sept. 15, 1872. A severe shock was felt at Yokohama, Japan.

Sept. 21, 1872. A slight shock was felt in the afternoon at Shanghae, China, lasting several seconds.

Sept. 28, 1872. A smart shock was felt at Lima and neighboring towns in Peru.

Oct. 2, 1872. A shock of three seconds' duration was felt in the morning at San Francisco, Cal.

Oct. 9, 1872. A severe shock was felt about 10 A. M. at Sioux City, Iowa, and at Yanctou, Fort Randall, and other points in Dakota. At Sioux City its duration was "twenty or thirty seconds." Though distinctly felt on the low grounds, it was not noticed on the bluff. At Fort Randall it was more severe, and at White Swan, Dak., it was accompanied by a sound like distant thunder.

Oct. 12, 1872. A severe shock was felt at Oakland and San Francisco, Cal., at 4.9 A. M. The Oakland Transcript of Oct. 14 says: "Persons who were awake at the time all agree that the first motion was a vertical one, and immediately after there was a swaying of the earth, the vibrations being from southeast to northwest, or following the range of hills to the northward of the city. It was felt very severely along their base, but no material damage was done anywhere." At San Francisco there were two shocks of several seconds' duration, with oscillations from east to west.

Oct. 18, 1872. Sharp shocks were felt in many districts of New South Wales.

Nov. 12, 1872. A sharp shock was felt in the night at Austin, Nev. A light shock was felt at Stockton, Cal., the same night. A shock was also felt at Valparaiso, Chili, on the same date.

Nov. 18, 1872. A severe shock occurred about 2 P. M. at Concord, N. H. It lasted about ten seconds. The shock was distinctly heard and was plainly perceptible to persons walking in the streets. The apparent course was from west to east. The shock was felt in adjacent towns and also at Laconia, about thirty miles north.

Nov. 28, 1872. A slight shock was felt at Derby, Eng.

Dec. 1, 1872. Despatches of this date from City of Mexico state that shocks had been felt in the district of Michoacan, that a new volcano was forming and eruptions were frequent. Previous letters from Gaudalaxara say that since the end of March last the volcanoes of Colima and Soborneo had shown unusual activity. Loud subterranean sounds were heard, with severe earthquakes and partial eruptions, setting fire to forests and plantations.

Dec. 10 and 11, 1872. Severe shocks were felt at Helena and Deer Lodge, Montana.

At Helena, at 4½ P. M. Dec. 10, there were two shocks of about equal force and duration, the two lasting five seconds. The direction of the wave was from west to east, and it was accompanied by the usual rumbling noise. The vibration was sufficiently forcible to crack plastering, part stove pipes, &c. At 7 A. M. Dec. 11, there was another shock nearly as forcible as the first, but no sound was heard.

At Deer Lodge, which is separated from Helena by a range of mountains, the first shock was at 4.58 P. M. and the last at 6.55 A. M., and there was a slight shock between 2 and 3 o'clock A. M. Here also the wave was from the west or a little north of west and the rumbling noise was heard. It was rather more forcible than at Helena, and was more noticeable on the lowlands than on the bench land. At the time of the first shock it was perfectly calm, the sky one-third cloudy, thermometer +21°, barometer 25·814 in. The shocks were felt at other places in the same valley.

Dec. 14, 1872. A slight shock was felt along the Columbia river in Oregon and Washington Territory. The time is variously reported from 9.20 to 9.40 P. M. At Portland it lasted about 15 seconds. At Dalles there were four or five shocks, and another at 9 A. M., Dec. 15. At Umatilla there were three shocks. At Walla-Walla two heavy shakes. At Wallula a heavy shock, followed by five lighter ones at intervals of 15 minutes, after which a heavy rumbling sound was heard, and slight shocks continued at irregular intervals until 4 A. M.

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l damage was done anywhere." At San Francisco there
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st to west.

ART. XXVIII.—*On some points in Dynamical Geology;* by T. STERRY HUNT, LL.D., F.R.S.

IN his late essay on *The Formation of the Features of the Earth's Crust,* in this Journal for November and December, 1872, Prof. Joseph LeConte has discussed a wide range of subjects in geological dynamics, in a manner for which the geological student cannot but be grateful. After a consideration of the arguments with regard to the nature of the earth's interior, he arrives at the conclusion, that "the whole theory of igneous agencies—which is little less than *the whole foundation of theoretic geology—must be reconstructed on the basis of a solid earth;*" a conclusion which forms the starting point of his essay. It is here to be noted, that the late William Hopkins, to whom we owe one of our great arguments in favor of a solid globe, did not take this ground, but sought to explain the phenomena of igneous action by the hypothesis of portions of matter still remaining unsolidified at no great depth between the solid nucleus and the superficial crust. Dissenting from this view, though accepting the general conclusions of Hopkins and others as to a solid globe, I have been endeavoring, since 1858, to reconstruct, in the language of Prof. LeConte "*the theory of igneous agencies on the basis of a solid earth.*" Alone up to this time, so far as I am aware, I have labored to expand, complete and give geological and chemical consistency to the suggestion long since put forth, both by Keferstein and by Sir John Herschel, that the deeply-buried and water-impregnated strata between the superficial crust of the earth and the solid nucleus constitute a region "of plastic material adequate to explain all the phenomena hitherto ascribed to a fluid nucleus," since "any changes in volume resulting from the contraction of the (solid) nucleus, would affect the outer crust through the medium of the more or less plastic zone of sediments precisely as if the whole interior of the globe were liquid."

A softening by heat of previously solid porous sediments, filled with water, was maintained (in accordance with the views of Babbage as to the rise of the isogeothermal horizons from the deposition of newer strata,) to depend upon the accumulation of large thicknesses of sediment, the results of which were declared to offer a "*ready explanation of all the phenomena of volcanoes and igneous rocks.*" This relation of volcanic phenomena to great accumulation, and those of recent times to more modern sedimentary deposits, which was also maintained by me, was subsequently insisted upon and enforced by Prof. James Hall in the introduction to the third volume of the Paleontology of New York. A summing up of these views as put forth by me in the Canadian Journal in March, 1858, and in the Quarterly Geological Journal for November, 1859, will be found in this Journal for May, 1861 * (II, xxxi, 411). In this last it was shown, in opposition to the notion of Babbage (who had speculated upon the *expansion* and consequent *elevation* of the deeply buried strata by heat) that one of the effects of heat and water upon the buried sediments would be *condensation*, from the diminution of porosity and still more from the conversion of the earthy materials into crystalline species of higher specific gravity, thus causing *contraction* of the mass. A further and very important result of this accumulation there pointed out was by the softening of the underlying floor, or the "*bottom strata to establish lines of weakness or of least resistance in the earth's crust, and thus determine the contraction which results from the cooling of the globe to exhibit itself in those regions, and along those lines where the ocean's bed is subsiding beneath the accumulated sediments.*" Hence, I added, "we conceive the subsidence invoked by Mr. Hall, though not the sole nor even the principal cause of the corrugations of the earth's strata, is the one which determines their position and direction by making the effects produced by the contraction, not only of sediments but of the earth's nucleus itself, to be exerted along the lines of greatest accumulation." As farther results of this process of accumulation, I also asserted "the metamorphism of sediments *in situ*, their displacement in a pasty condition from igneo-aqueous fusion as plutonic rocks, and their ejection as lavas, with attendant gases and vapors." [Quart. Geol. Jour., Nov., 1859.]

With these conclusions, enunciated in 1858–1861, we may compare those arrived at by Prof. LeConte in his recent essay, where he recognizes as consequences of the heating of great thicknesses of sediments accumulated along the shores of a continent, a process of *condensation* in the lower strata, resulting in "contraction and subsidence *pari passu* with the deposit," followed by "aqueo-igneous softening or even melting, not only of the lower portion of the sediments themselves, but of *the underlying strata upon which they were deposited;* the subsidence probably continues during this process. Finally, this *softening determines a line of yielding to horizontal pressure*, and a consequent upswelling of the line into a chain. Thus are accounted for first, the *subsidence*, then the *subsequent upheaval*, and also the *metamorphism* of the lower strata." Beneath every great line of sediments there will moreover be found, according to him, a reservoir of sedimentary material in a state of more or less complete fusion, in which volcanic phenomena have their seat. The reader cannot fail to see that these views are identical with those which I have so long advocated.

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* See also, On the probable seat of Volcanic Action, this Journal, II, l, 21, and Geol. Mag., June, 1869.

The views of Prof. James Hall as to the relation between great accumulations of strata and mountain-elevations, are cited with approval by LeConte, who, following him, asserts that "mountain chains are masses of immensely thick sediments." I venture, however, to remark, in this connection, that the views both of Mr. Hall and of myself, as his expounder, have as yet been but imperfectly understood either by LeConte or our other critics. Thus they have been defined as "a theory of mountains with the origin of mountains left out;" while LeConte says, "Hall and Hunt leave the sediments just after the whole preparation has been made, but before the actual mountain-formation has taken place." Now, in fact, so far as I am aware, neither Hall nor yet myself in my exposition of his views, which will be found in this Journal for May, 1861 [II, xxxi, 406–410], has proposed any theory to explain this latter part of the process, that is to say, the uplifting of the deposited sediments, which LeConte calls "the actual mountain-formation." Hall's contribution to the problem, which, as our author well says, forms "an era in geological science," was to show the relation between mountain chains and great accumulations of sediments, to illustrate this by the history of the Paleozoic rocks of North America; and moreover to protest against the generally received theory that mountain elevations are due to local upthrusts; he, to use his own language, "going back to the theories long since entertained by geologists relative to continental elevations." That mountains were the remnants of eroded continental areas had already been taught by Lesley, and long before by Buffon and DeMontlosier. It was left for Hall, through a new way, to lead us back to these views; but the whole theory of the cause of continental elevations was left by him where he found it. In my exposition of his views, I have only endeavored, in addition, to show in what manner a contracting globe and a solid nucleus may be related to the great facts of local subsidence and accumulation.

I shall not attempt to follow LeConte in his objections to the views of Dana and Whitney with regard to the uplifting of mountains, but proceed to notice briefly his own, according to which the horizontal thrust resulting from the slow contraction of the nucleus is brought, in the manner which I long since explained, to act upon the great accumulations of sediment, so that they are "crushed together horizontally and swelled up vertically," thus producing not only plications and slaty cleavage, but an amount of vertical extension "*fully adequate to account for the upheaval of the greatest mountain* *chains;*" the ridges, peaks, gorges and valleys of mountain regions being however the results of subsequent erosion. This theory of the plications of strata, and their relations both to great accumulations and to a contracting nucleus, is fully set forth in my paper of 1861, already quoted; where I have also insisted upon the results of "the lateral pressure brought to bear upon the strata in an elongated basin (of subsidence) by the contraction of the globe." But while admitting that the process here described must cause elevations of the compressed strata, it must be said that it fails to solve the problem of the uplifting of mountain regions, the strata of which have, in many cases, undergone neither folding nor lateral compression, but are nearly or quite horizontal. Foldings, contortions and slaty cleavage, though met with in many mountain regions, are in fact, accidents which are to be left out of view in considering the origin of mountains. The student of physical geography may learn from the great elevated plateaus of the globe, the truth of De Montlosier's statement, that the great mountain chains of Europe are but the remains of continental elevations which have been cut away by denudation, and that the foldings and inversions to be met with in the structure of mountains are to be looked upon only as local and accidental. [This Journal, II, xxxi, 408.] In a similar spirit Jukes remarks that we learn "how completely the present surface of the earth is a sculptured surface carved out by denudation, and how little, as a rule, it is effected by the dislocations, upheavals and convolutions of the rocks beneath it." [Manual of Geology, 3d ed., 449.] In the case of the uplifted paleozoic basin of eastern North America, as Hall has well shown, the process of elevation was the same for the thicker and corrugated sediments of the eastern portion and for the thinner and undisturbed strata of the valley of the upper Mississippi. The hills in the latter region, built up of 1,000 feet of horizontal beds, having the Potsdam sandstone at the base, and capped by the Niagara limestone, show us the production of mountains by erosion, uncomplicated by the accidents which make their study more difficult in regions where contortion of the strata has supervened. Hall has also noted in this connection the nearly horizontal strata of the Cattskill Mountains.

The question of the structure and the origin of the Appalachians has been complicated by the assumption that the crystalline strata which constitute their higher portions are altered sediments of paleozoic age, rather than parts of an ancient continent of eozoic rocks which formed the eastern border of the paleozoic sea, corresponding to the Rocky Mountains on the west. The former view has been very generally held by American geologists, and was maintained by the present writer until 1870, when he endeavored to show that the crystalline rocks of New England and their lithological representatives both to the southwest and the northwest are of pre-paleozoic age, and in part Laurentian. [This Journal, II, 1, 83; also Address before the American Association, Indianapolis, 1871.] This view, already before maintained by Credner and by Emmons, is now advocated by LeConte, who conceives that the gneissic region of the Atlantic slope is Laurentian, and was probably "land during the paleozoic times," constituting an "eastern continental mass," which, judging from the immense thickness of sediments in the eastern parts of the paleozoic area, must have been of great extent. A similar view was put forward by H. D. Rogers in 1842, and again by James Hall in 1859, when, after describing these paleozoic sediments, he said, "we may have had a coast line nearly parallel to and co-extensive with the Appalachians" [Paleontology, iii, page 96, note]; commenting upon which, in 1861, I asserted that these coarse sediments "were evidently derived from a wasting continent." In a paper read before the American Geographical Society, New York, Nov. 12, 1872, I adduced a farther argument in favor of such a pre-paleozoic continent to the eastward, from the climatic condition of great dryness which gave rise to the paleozoic region of North America, to deposits of salt, gypsum and dolomite over considerable areas from Nova Scotia to Michigan and Ohio, and from the time of the Calciferous sandrock to that of the Lower Carboniferous. [Engineering and Mining Journal, Jan. 14, and 23, 1873.]

In concluding his essay, Prof. LeConte declares that an important problem in geological dynamics remains, in his opinion, unsolved; viz., the cause of those "great and wide-spread *oscillations* which have marked the great divisions of *time* and have left their impress in the general unconformability of the strata;" the last being that of the post-tertiary period. Now it is precisely the *upward* movements of this kind which constitute the continental elevations of De Montlosier, Hall and myself, giving rise to plateaus, and by the partial erosion and denudation of these to mountains. The cause of these continental elevations was not discussed by Hall, and is by LeConte declared to be unexplained; while such is the case, "the actual mountain-formation," to use his words, is still unaccounted for. That these gentle and wide-spread movements of oscillation are, however, in some way not yet clearly explained, connected with the contracting of the nucleus and the consequent conforming thereto of the envelope, we can scarcely doubt, or that the latter, from its nature and origin, must present great differences in constitution and in flexibility in its various parts.

From this it might be expected that the movements imparted to the envelope alike by the process of secular cooling and contraction of the nucleus, and by the disturbance of the equilibrium of pressure consequent on the processes of erosion and sedimentation would give rise to seemingly irregular oscillations, resulting in the depression or the elevation of considerable areas, constituting continental movements.

The grave question here arises as to whether the heat which plays such an important part in the phenomena under consideration, is a cause or an effect of the activities beneath the earth's surface. Starting from the notion of an igneous center, Babbage and Herschel adopted the first view, in which I have followed them, maintaining that the heat from a yet uncooled nucleus is the efficient cause of all igneous and volcanic manifestations. According to Keferstein, on the other hand, the hypothesis of an incandescent nucleus is unnecessary, and the internal heat results from what he called a fermentation among the deeply buried sedimentary layers. A view which unites these two is proposed by LeConte, who suggests that heat from a central source invading the buried sediments may there excite chemical action, which will, in its turn, evolve heat, and thus greatly augment their temperature. It is, however, I think, probable that any chemical processes which may be set up in the buried sediments for their conversion into igneous rocks and volcanic products would absorb rather than generate heat.

In his remarkable study of the Secular Cooling of the Earth [Trans. Royal Soc. Edinb., xxiii, pt. 1, page 157], Sir William Thomson arrives at the conclusion that the observed mean rate of increase in descending in the earth's crust will continue with but little variation for 100,000 feet, but will gradually diminish at greater depths. Estimating with him the rate of increase at one degree of Fahrenheit for fifty-one feet, it would require the depth just named, or about nineteen miles, to give a temperature of 2,000° F., which may be supposed sufficient to produce the chemical actions required. See my paper on The Probable Seat of Volcanic Action. [This Jour., II, l, 21.] But it is probable that the seat of volcanic activity may be much less profound than above supposed, in which case the central heat would be inadequate. *Chemical* action, as suggested by Keferstein and LeConte, being rejected as a source of heat, there however remained the hypothesis that thermal effects might result from local *physical* causes, and that the immense chemical force exerted in the movements of the earth's crust might be converted into heat. This view was, so far as I am aware, first advanced by Mr. George L Vose,* whose tion and of limited applications when compared with the very simple apparatus I will now proceed to describe.

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* As recognized by Prof. LeConte, on page 156, this volume.

G is the glass shade of the galvanometer, on which, at *g*, are drawn in india-ink the vertical graduation lines of the instrument. A is a piece of aluminum wire to whose lower end are fixed the needles of the galvanometer, and whose upper end is perforated with a small hole so that the system may be suspended by a silk fiber. A fine wire of german-silver, *w*, is attached transversely to the aluminum wire, and has its ends bent downward at right angles to its length. This transverse wire can be placed at any azimuth by rotating it around its center, which is coiled two or three times around the vertical aluminum wire. On one of the bent ends of the transverse wire is cemented a diamond-shaped piece of light paper or foil, and the other end carries a small ball of wax, whose weight equals that of the piece of paper or foil. The diamond courses around the shade at about one millimeter from its interior surface, with its lower point just above the lines of graduation. At C are represented the condensing lenses of an oxhydrogen lantern, whose jet and lime are at L. O is the objective which gives on the screen the magnified image of pointer and scale, as seen at S. This scale is not graduated into equal angular divisions, but its units represent units of deflecting force traversing the galvanometer; and this scale is derived from a careful calibation of the instrument.

The sharpness of the image on the screen is admirable, and with the calcium light is distinctly visible in a room considerably illuminated with daylight. With less illumination of the room I have used the instrument when the calcium light was replaced by a kerosene flame.

Evidently the precision of the indications of the apparatus just described are vitiated by the parallax of the index, for it does not describe a cylinder which is an extension of the one on which are drawn the graduations. This error is avoided by cementing on the inside of the shade a curved piece of glass whose radius of curvature equals the arm carrying the index, and whose center coincides with the axis of the aluminum wire. With this modification in the apparatus we have succeeded in reading with precision deflections to 6′ of arc.

By the following arrangement deflections to 1′, in an arc extending 5° each side of the 0° point, can be determined. A thin slip of microscope cover-glass is coated with a layer of black varnish, and through this varnish are cut, in a dividing engine, fine equidistant lines. The diamond-shaped pointer is replaced by a light piece of cover-glass, also coated with varnish, and having cut on it one fine vertical line. These lines are illuminated by the lantern, and in front of them is placed an inch or an inch and a half objective. On the screen we have the graduations as a series of bright lines on a dark ground, and along them moves the bright index line of the pointer.

The zero points of the scales can be brought accurately to coincide with the normal position of the index by revolving the shade on its base; and by turning the transverse wire so that it points toward the screen, when the needles of the galvanometer have come to rest, we can readily project the image of the pointer and scale in any desired direction.

Although there are some advantages in having the scales attached to the galvanometer, and in obtaining on the screen their magnified images; yet, we can save much time in the construction of the apparatus by substituting for them scales drawn directly on the screen in very black india-ink.

My experience with this instrument has led me to prefer the use of only one magnetic needle—the one enclosed in the coil of the galvanometer—and this needle I render more or less astatic by means of a damping magnet placed above the galvanometer and sliding on a vertical rod and rotating on its center around the same. By means of the magnet, one can, with expedition, adapt the sensitiveness of the instrument to the requirements of special experiments, and thus the instrument is admirably suited for all experiments in radiant heat, electricity or magneto-electricity. In fact, on holding my hand at a distance of 15 centimeters from the face of a thermo pile, without its conical reflector, I have often deflected the needle 30°.

One might suppose that the heat from the source of light would cause currents of air in the shade and make the needle fluctuate; but I have not met with this difficulty, and if it should arise it can be removed by placing in front of the condensing lenses a glass tank containing a solution of alum.

The evident superiority of this instrument in simplicity of construction over all preceding devices will at once commend it; indeed, anyone in an hour or so can convert his galvanometer into a more convenient and precise instrument for research, and for the illustration of phenomena which have heretofore been presented to students only with considerable trouble on the part of the professor. Now, however, the whole of the beautiful phenomena of radiant heat, which require a delicate instrument for their evolution, can be exhibited with ease and pleasure before our college classes.

December 31st, 1872.

ART. XXX.—*Investigations on Parasulphobenzoic Acid;* by IRA REMSEN.

(Continued from page 186.)

III. *Formation of Parasulphobenzoic Acid from Sulphotoluenic Acid.*

WHEN substituting agents are allowed to act upon toluene, in the case of derivatives containing one substituting group, two products are formed. These are the para- and ortho-varieties. The former is always produced in much larger quantity than the latter. This has been proved by Engelhardt and Latschinoff,* and by Wolkow,† in the case of the sulpho-derivatives. Wolkow proved that the products belonged to the para- and ortho-series by converting them respectively into paraoxybenzoic and salicylic acids. According to the experiments of the chemists mentioned, the two sulpho-acids can be separated by partial crystallization of the potassium salts. In this way the parasalt was obtained in a perfectly pure condition, while the ortho-salt could not be freed entirely from the para-salt.

Now, as it had been shown that the oxidation of the monobrom- and mono-chlor-derivatives of toluene yielded corresponding derivatives of benzoic acid, the idea naturally suggested itself that similar treatment of sulphotoluenic acid might yield the desired derivative of benzoic acid. My best hopes were satisfied. After a few experiments it became evident that by means of this reaction I could prepare parasulphobenzoic acid in unlimited quantity with but comparatively little labor.

The principal difficulty that presented itself was to be looked for in connection with the fact that the sulpho-acid is exceedingly easily soluble; and some method had to be devised to extract it from the oxidizing mixture, provided it were formed. A preliminary experiment made with a small quantity of the potassium salts of the sulphotoluenic acids showed that they were easily acted upon by a mixture of sulphuric acid and potassium bichromate. Taking advantage of the suggestion of Fittig,‡ with the hope that the ortho-acid would be destroyed by the oxidizing agents, I subjected the mixture of the two potassium salts to the influence of sulphuric acid and potassium bichromate in noted proportions. The oxidation, when once commenced by gently heating over a water-bath, proceeds rapidly to the end without the further aid of heat, and is completed in the course of a few minutes. The liquid becomes very hot and foams somewhat; an evolution of gas takes place as long as the oxidation-process continues; and its cessation indicates the end of the operation. In order to extract the product from the mixture the whole was diluted with a large amount of water, and chalk added to the point of neutralization. By this means the chromium oxide formed, and the excess of sulphuric acid were precipitated, and in the solution remained the potassium salts of the sulpho-acid or acids, together with some neutral potassium chromate. Baryta-water was now added in sufficient quantity to precipitate exactly the chromic acid present, this filtered off, and the filtrate evaporated almost to dryness. The colorless residue consisted of the potassium salts together with some potassium hydroxide. The mass was first neutralized with sulphuric acid, and then a sufficient quantity of the latter added to set the sulpho-acids free, care being taken to avoid any large excess. Moderately strong alcohol being now poured upon the mixture, an abundant deposite of ptassium sulphate took place. This was filtered off, the salt well washed out with alcohol, and the alcoholic filtrate evaporated down again to a small volume. Potassium sulphate was again deposited. This was filtered off, washed out, etc., and the operation repeated a few times. Finally the alcoholic solution was boiled for some time with water, and then evaporated to dryness over the water-bath. In this way the sulpho-acids were obtained in a free state without impurities. The acid barium salts were prepared, and, on bringing their solution to the point of crystallization, large acicular crystals were at once deposited, and these possessed the characteristics of the acid barium salt of parasulphobenzoic acid already described. They were analyzed with the following results:

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* Zeitschrift für Chemie, N. F. 5, 615. † Ibid., N. F. 6, 321.

‡ Zeitschrift für Chemie, N. F. 7, 179.

0·5415 grams salt were heated above 200°, and lost 0·0495 grams H^2O; and gave 0·2130 grams $BaSO^4$=0·12524 Ba.

	Calculated.		Found.
$(C^{14}H^{10}S^2O^{10})$	402	67·79	
Ba	137	23·10	23·13
$3H^2O$	54	9·11	9·14
	593	100·00	

This shows then conclusively that by the oxidation with sulphuric acid and potassium bichromate, the sulpho-group of parasulphotoluenic acid remains intact. The conduct of the ortho-acid under like circumstances I shall refer to below. As far as I have been able to discover by a consultation of the literature, this is the first attempt which has been made to oxidize sulpho-acids in the manner described. The simplicity of the reaction and the satisfactory character of the results lead me to desire the further application of the principle involved, and I shall take the first opportunity to prepare a pure sulphoxylenic and sulphosmesitylenic acid with the object of subjecting them to the influence of oxidizing agents, hoping thus to obtain an *oxybibasic* and an *oxytribasic* acid.

After having gained the necessary preliminary knowledge, I proceeded to determine the best conditions for the reaction. A large number of experiments were made, and as the result I would give the following directions: Instead of first preparing the potassium salts of the sulphotoluenic acids, I employed a solution of the acids in sulphuric acid, considerable labor being thus saved. 25 grams of pure toluene are dissolved in 200 grams of fuming sulphuric acid without the aid of heat. When this solution has cooled down somewhat, two volumes of water are added and the height of the liquid in the flask marked. Now more water is added, and the mass subjected to distillation until the liquid has reached the original volume indicated by the mark on the vessel. In this way any toluene which may have remained unacted upon by the sulphuric acid is removed. The solution is now allowed to cool to the ordinary temperature, and then 160 grams of coarsely powdered potassium bichromate gradually added. In order to start the oxidation the flask is placed on a water bath, and gently heated for about ten minutes. During this time a commotion is noticed in the liquid which gradually increases. An active foaming ensues, and when this has fairly begun, the flask is removed from the water-bath. A uniform evolution of gas continues until the end of the operation, which occupies usually about twenty minutes in all. The gas evolved is carbonic acid, as was proved by appropriate reactions. In heating the mixture at first, it is absolutely necessary to place it on a water-bath; if the attempt be made to heat with a flame the flask invariably breaks. This is occasioned by the fact that the potassium bichromate lies at the bottom of the flask; and that the oxidation commences and goes on rapidly just at the spot where the heat from the flame is strongest. This spot immediately becomes very hot before the remainder of the glass has been at all heated, and from this spot a circular piece of glass inevitably drops, followed by the contents of the flask. When the operation is at an end, which, as stated, is indicated by the cessation of the evolution of gas, the whole is diluted with water, and then treated successively, as above described, with chalk, baryta-water, sulphuric acid and alcohol. By this method in the course of a few days a very large quantity of pure acid barium parasulphobenzoate can be prepared.

Parasulphobenzoic Acid, $C^6H^4 \left\{ \begin{array}{l} SO^2.OH \\ CO.OH \end{array} \right.$, is prepared from the barium salt by precipitating the barium exactly with pure sulphuric acid, and evaporating the solution. It is very easily soluble in water, and crystallizes from a very concentrated solution in the form of beautiful, colorless, transparent needles. These, though very easily soluble, are not deliquescent. They fuse above 200°, but undergo decomposition before the fusing point is reached. The meta-acid is deliquescent.

Potassium Parasulphobenzoate, prepared by neutralizing and precipitating the acid barium salt by means of a solution of pure potassium carbonate, is exceedingly easily soluble in water, but crystallizes finally in well-formed, transparent needles.

Acid sodium parasulphobenzoate, $C^6H^4 \left\{ \begin{array}{l} SO^2.ONa \\ CO.OH \end{array} \right. + 2\frac{1}{2}H^2O$. This salt was prepared by neutralizing and precipitating the acid barium salt with sodium carbonate, and then adding hydrochloric acid to the solution, evaporating and allowing to crystallize. It forms beautiful, long, colorless, lustrous, stellate prisms. It is moderately easily soluble in cold water, more easily in hot water. The corresponding salt of the meta-acid is more difficultly soluble in cold water, and crystallizes in laminæ. The two, when present in the same solution, can not, however, be separated. The analysis of the salt gave the following results:

0·3707 grams of the salt, dried over sulphuric acid, on being heated gradually to 310°, lost 0·0607 grams in weight; and then gave 0·102 grams Na^2SO^4=0·033038 grams Na.

	Calculated.		Found.
$(C^7H^5SO^5)$	201	74·72	
Na	23	8·55	8·91
$2\frac{1}{2}H^2O$	45	16·73	16·37
	269	100·00	

The remarkable fact will be noticed that the water of crystallization is not driven off entirely until a high temperature (320°) is reached. All other salts of this acid, as well as of the meta-acid, which contain water of crystallization, exhibit the same property, though not in such a marked degree as this one.

Barium parasulphobenzoate, $C^7H^4SO^5.Ba+2H^2O$. This salt was obtained by neutralizing a solution of the acid salt with barium carbonate. It is moderately easily soluble in cold water, very easily in hot water. It crystallizes in small needles, which are grouped together in verrucous masses. The corresponding salt of the meta-acid is also easily soluble in water, but according to the descriptions given it contains no water of crystallization.

The analysis resulted as follows:

0·4174 grams salt, dried over sulphuric acid, on being heated gradually to 190°, lost 0·0411 grams H^2O; and then gave 0·2587 grams $BaSO^4$=0·15212 grams Ba.

	Calculated.		Found.
$C^7H^4SO^5$	200	53·62	
Ba	137	36·73	36·44
$2H^2O$	36	9·65	9·84
	373	100·00	

Acid barium parasulphobenzoate, $(C^7H^5S^2O^5)^2Ba+3H^2O$. The methods of preparation and analysis of this salt have already been given in detail. It is by far the most characteristic salt of parasulphobenzoic acid; and its properties are such as to render its preparation in an absolutely pure condition very simple. It is exceedingly difficultly soluble in cold water, When perfectly pure the length of the crystals is only dependent upon the depth of the liquid in which they are formed. It is more difficultly soluble, both in cold and in hot water, than the meta-salt. Like the meta-salt it does not give off its water of crystallization entirely below 200°; and it may be subjected to a much higher temperature without the danger of decomposition.

Calcium parasulphobenzoate is an amorphous powder which is somewhat more easily soluble in cold water than in hot, and is hence thrown down when a concentrated cold solution is boiled.

When the potassium salts, obtained in the preparation of acid barium parasulphobenzoate by evaporating the solution which has been treated with chalk and baryta-water, are fused with potassium hydroxide, a mixture of paraoxybenzoic and salicylic acids is obtained, the salicylic acid forming in some cases fully half of the product. This fact taken alone led at first to the conclusion that the methyl groups of both the para- and ortho-sulpho acids had been oxidized; and that thus not only parasulphobenzoic acid had been formed, but at the same time orthosulphobenzoic acid. Further investigation, however, showed conclusively that this was not the case, but proved another interesting fact, of which I shall speak below.

IV. *Formation of Terephtalic Acid from Parasulphobenzoic Acid.*

The recent experiments of V. Meyer* have tended to materially modify the prevalent views in regard to the constitution of the biderivatives of benzene. Meyer showed that ordinary sulphobenzoic acid, which, on the one hand, could be converted into oxybenzoic acid, could, on the other hand, be converted into isophtalic acid by fusing its potassium salt with sodium formate. As, according to the reigning ideas, isophtalic acid can only have the constitution indicated by the 1·3 position of its carboxyl groups, it became evident that oxybenzoic acid, which up to that time had been looked upon as belonging to the same series as phtalic acid, viz: the ortho (1·2) series, in reality belonged to the meta (1·3) series, of which isophtalic acid is the most satisfactory representative. Salicylic acid thus became the 1·2 oxybenzoic acid, and the formulæ of a number of compounds were subsequently changed of necessity to place them in concordance with the results of the above reaction. But to take thus one experiment as the basis of a change as serious as that which ensued was looked upon by some chemists as insufficient; and, indeed, Meyer himself, in his first notice* on this subject, says: "Bei allen Schlüssen, die wir aus Reactionen, wie die oben beschriebene, ziehen, mahnt freilich die von Kekulé beobachtete Thatsache, dass die Phenolsulfosäure mit Leichtigkeit aus der Meta-Stellung in die Para-Stellung übergeht, zu grosser Vorsicht und ich werde daher auch die so eben aufgestellte Reihe nicht für völlig bewiesen halten, bevor ich nicht auch ein Glied der Meta-Rahe in gewöhnliche Phtalsäure werde übergeführt haben." Notwithstanding the fact that a great number of experiments were made with the object of more firmly establishing the principle adopted, by converting a member of the other series into the corresponding bibasic acid, they all failed; and the two analogous experiments of Meyer, viz.: the conversion of sulphobenzoic acid into isophtalic acid and the conversion of bromobenzoic acid into isophtalic acid, remained without support in their testimony. Attempts to apply the reaction to other fields were also unsuccessful, as shown in the experiments of Barth† and Ascher.‡ It is hardly strange then that, with these circumstances, the changes proposed by Meyer were not universally accepted; and those who opposed them on the ground that molecular rearrangement might here play a role were certainly to some extent justified.

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* Berliner Berichte, III Jahrgang, 112; and Annalen der Chemie u. Pharmacie, clvi, 265.

As I was now in possession of the para-acid§ corresponding to the meta-acid‖ with which Meyer performed his experiment, it became an interesting question as to what the conduct of this compound would be when fused with sodium formate.

From the pure acid barium salt the potassium salt was prepared and, the directions of Meyer being closely followed, this salt was fused with an equal weight of pure sodium formate. In order to bring the mass to the point of fusion a comparatively high temperature was required. It then remained in a semi-liquid condition, apparently evolving gas for a short time, finally becoming much darker in color—in fact nearly black. At a certain point volatile products, evidently containing sulphur, were given off, the odor of which was intensely disagreeable. The operation was performed in a silver crucible; and the mass constantly stirred with a silver spatula. Occasionally the vapors which were given off took fire above the crucible, and, on the gas-flame being now removed from beneath, and the flame of the burning vapor being extinguished, the mixture continued red-hot for a short time, presenting the appearance of a burning coal. When all had cooled down to the ordinary temperature the crucible and contents were placed in water, and this boiled. The solution thus obtained was filtered and, when cold, was treated with sulphuric acid. Thus was thrown down a very voluminous, flocculent precipitate of a decidedly dark color. In order to purify the product, it was filtered off and well washed out with hot water; then dissolved in ammonia, and this solution boiled with animal charcoal. A nearly colorless solution resulted, and on treating this with sulphuric acid the precipitate formed was almost white. An attempt was made to prepare the barium salt by boiling with pure barium carbonate. After long continued boiling with a large amount of water the acid had disappeared and the salt was in solution. On evaporating gradually the salt was deposited in crusts during the process. It proved to be of exceedingly difficult solubility in water, boiling as well as cold. A small portion of it appeared to have a tendency to crystallize. This was separated from the powder and crusts, and repeatedly recrystallized. It was also very difficultly soluble in water, and yielded an acid which resembled terephtalic acid in some properties. By means of various reactions, however, it was soon proved that this was not one of the phtalic acids, and it seemed probable that it might represent a variety of the thihydrobenzoic acids, the formation of which has been shown* to take place in the reaction of Meyer for the preparation of isophtalic acid. The amount of the substance obtained was not sufficient to permit of its close examination, its perfect separation from the other substance formed being impossible. The difficulty of separation threatened at the outset to be a serious obstacle in the way of deciding the point under consideration. One method after another was tried; but the results were decidedly unsatisfactory; until finally the mixture was subjected to the influence of an oxidizing agent (sulphuric acid and potassium bichromate). By this means the thihydrobenzoic acid (?) was so changed in character as to become soluble, whereas the other constituent of the mixture was left behind in a pure condition unacted upon. It was dissolved in ammonia, reprecipitated by means of a strong acid, filtered and well washed out. In this condition it had the form of a very light, flocculent, white mass. It could be dissolved in boiling alcohol, and from this solution it was obtained in the form of microscopic needles which were deposited upon the sides of the vessel. This substance could not be brought to fusion. When heated in a capillary tube it sublimed from one part to the other before the flame; and was finally decomposed without fusing. It was almost absolutely insoluble in water, both boiling and cold; insoluble in ether. The pure substance could not be perfectly dissolved by boiling with barium carbonate. A small amount of the barium salt of the acid was, however, thus obtained, and this was very difficultly soluble in water and did not crystallize. The calcium salt resembled this in every way.

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* Berliner Berichte, III Jahrgang, 112.

† Berliner Berichte, IV Jahrgang, 634.

‡ Annalen der Chemie u. Pharmacie, clxi, 3.

§ Para 1·4.

‖ Meta 1·3.

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* Ador, Berliner Berichte, IV Jahrgang, 622.

These are the properties of terephtalic acid, with the exception of the conduct toward alcohol. To this I am not inclined to attach much weight, as the acid which is described as insoluble in alcohol is that which is obtained by oxidation of xylene, and the *condition* of this acid differs essentially from that of the light mass obtained by precipitating it from one of its salts. Further, I found that after being dried, the acid, as obtained by me, was also insoluble in alcohol. I would hence rather consider this conduct as indicating a property of terephtalic acid which had been overlooked. The substance was proved to have the composition of terephtalic acid by the following analysis:

0·2325 grams substance, dried over sulphuric acid, gave 0·4897 grams CO^2 = 0·13355 grams C and 0·0832 grams H^2O = 0·00924 grams H.

	Calculated.		Found.
C^8	96	57·83	57·44
H^6	6	3·61	3·97
O^4	64	38·56	
	166	100·00	

The proofs that terephtalic acid is formed when potassium parasulphobenzoate and sodium formate are fused together are thus conclusive. It remained, however, to show that neither phtalic nor isophtalic acid was formed at the same time. The crude product was boiled with water for a long time and then filtered off. On allowing the filtrate to cool a small quantity of substance was deposited in the form of powder. The whole was shaken with ether, which dissolved the powder and extracted whatever might be in solution. The original solution from which the crude acid had been precipitated was also treated with ether. On uniting the ethereal solutions and distilling off the ether, a residue was obtained which dissolved readily in alkaline carbonates. It was neutralized with barium carbonate. The barium salt was easily soluble and crystallized well. The free acid separated from this salt was easily soluble in hot water and crystallized out on cooling. It had the fusing point 120° and all the other properties of *benzoic acid*. No other substance could be found. The quantity of benzoic acid obtained was very small in comparison to the whole quantity of the product; and its formation can easily be accounted for when we consider the character of the reaction.

Here then, at least, no molecular rearrangement takes place; and this, taken in connection with Meyer's experiment, certainly makes the case strong enough to command attention. The reaction is thus shown to be capable of application for the purpose of determining the constitution of compounds; and the changes proposed by Meyer can be demanded with greater confidence than before. The proofs that paraoxybenzoic and terephtalic acids belong to the same series had already been given* by other reactions; though, acknowledging the described reaction, this would be the most direct proof of the fact.

(To be continued.)

ART. XXXI.—*Note on the Age of the Metamorphic Rocks of Portland, Dodge county, Wisconsin*; by ROLAND D. IRVING, E.M., Professor of Geology, Mining and Metallurgy, in the University of Wisconsin.

IN an Article on "The Age of the Quartzites, Conglomerates and Schists of Sauk County, Wisconsin," published in this Journal for February of the present year (1872), I gave what I believe to be ample proof of the Pre-Potsdam age of the rocks then treated of. These rocks, previously regarded by good authority (Winchell, Eaton, Percival), as having resulted from a metamorphism of the Potsdam sandstones, I then showed to have been outlying islands and reef ledges in the Potsdam seas. I stated also that there were several other outlying patches of metamorphic rocks, similar to these, and scattered at wide distances apart within the Lower Silurian area; one of these I have since been able to examine with some care, and am prepared to say of the rocks found there what I did of those in the Sauk county region, viz: that they are undoubtedly Pre-Potsdam. The locality referred to is near the village of Portland, in the

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* See V. Meyer, Annalen der Chemie u. Pharmacie, clvi, 267.

S.W. corner of Dodge county, at least thirty-five miles from the Sauk county metamorphic rocks, and fully eighty-five miles from the nearest point of the main Azoic body of the northern portion of the State.

I have ascertained, I think, all that has heretofore been published about this locality. The first public announcement seems to have been made by Dr. I. A. Lapham in a lecture at Milwaukee in February, 1848. He there speaks of the rock as just discovered and as "primitive rock." This lecture afforded Dr. Owen the data for the foot-note which I find on page 151 of Owen's Report on Wisconsin, Iowa and Minnesota (1852), and from which I take the following: "The late Mr. J. S. Thayer observed a locality of *granite* in Dodge county, Wisconsin, on the west branch of Rock river, on Section 33, Township 9 north, Range 13 west." The only other reference I am able to find occurs in the Report of Progress of Dr. James G. Percival for 1855. On page 101 of that report I find a heading, "The Quartz Rock of Baraboo and Portland." The first sentence under this head reads as follows: "The quartz rock in the ridges adjoining the Baraboo valley, on the north and south, and that east of Portland, are so similar in character that they may be considered in connection." He further states that the Baraboo (Sauk) quartzites are changed "Lower" or Potsdam sandstone, whilst the Portland rocks in like manner result from the "Upper" or St. Peter's sandstone. In Mr. Hall's Reports, I find no mention of the Portland rock.*

The formations immediately surrounding the locality in question are of Potsdam age, but, inasmuch as these rocks are much farther south than any of the similar isolated masses, we find now in the immediate vicinity not merely the sandstone, or lower representative of the Potsdam period, but also, and mainly, the Lower Magnesian limestone or upper representative of that period. We find too, within a very short distance, the St. Peter's or "Upper" sandstone, and the Blue and Buff limestones, all of the Trenton period. In this case, then, the occurrence is even more strikingly peculiar than in the Sauk county region. Here we find a very much smaller area covered by the metamorphic rocks; these rocks are much further from the main Azoic mass, and the series of surrounding and entirely unaltered and undisturbed strata is much fuller. The accompanying map, enlarged from Dr. Lapham's Geological Map of Wisconsin, serves to show the association of the quartzite with the undisturbed Silurian rocks.

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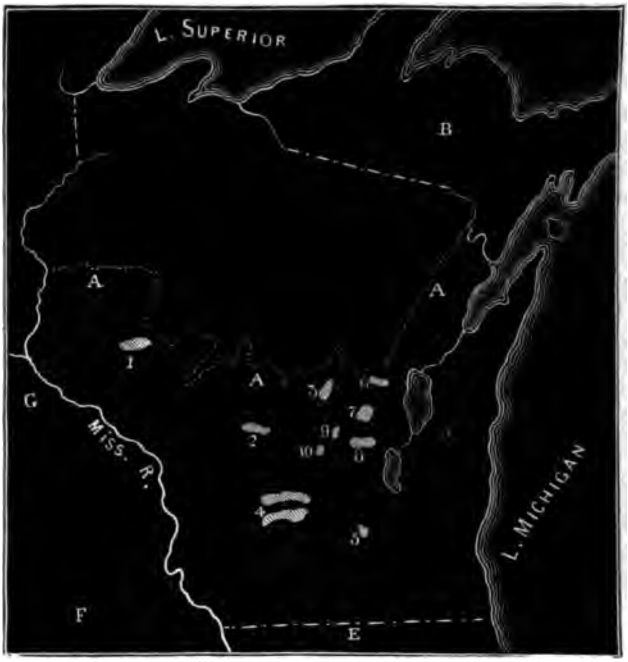
* I find by studying more carefully Mr. Hall's Report of Progress of the Geological Survey of Wisconsin, for 1861, that he gives more proof of the Huronian or Azoic age of Sauk quartzites than I supposed when I wrote the article alluded to above. He may have had more proof in reserve for the final report, which reached only one volume before the survey was stopped. In the plate of sections given in that volume, the Sauk quartzites are represented *as rising in a series of anticlinal folds, instead of having a uniform dip to the north.* This shows that the ideas then entertained with regard to these rocks must have been erroneous.



A, A, A. Southern limit of Azoic rocks. 1, 2, 3, 4, 5, Masses of quartzite scattered within the Lower Silurian areas. 4, Sauk county Quartzites. 5, Portland, Dodge county, Quartzites. 6, 7, 8, 9, 10, Granite masses within the Lower Silurian area (according to Lapham). B, Northern Michigan Peninsula. C, Lake Superior. D, Lake Michigan. E, Illinois. F, Iowa. G, Minnesota.

The quartzite mass here covers an area of not more than three miles in an east and west direction, and much less than that in a north and south direction. On approaching from the east the rocks appear in the form of a low ridge, whose height in no case exceeds 75 feet above the general level of the country. The approach is across a low marshy ground, and the ridge though not high is thus made to stand out somewhat conspicuously. This marshy ground runs along all the western side and at the northern end completely encircles one portion of the ridge, making a marsh island of it. The junction of two sluggish marsh streams is near by, and in times of high water this island in the marsh is actually surrounded by water and accessible only by boat. It goes by the name of "Rocky Island." The highest point of the ridge, as well as the most marked exposures of rock, is found at this place. The ridge is very narrow from east to west, and descends almost immediately on the eastern side to a shallow valley. The more eastern exposures are on a corresponding low ridge farther to the east, and are of less extent.

The rock is almost entirely quartzite. In a few places I found outcrops of a metamorphic conglomerate like that observed in Sauk county, and in still fewer places very thin seams of a talco-siliceous schist are visible. I found none of the siliceous slate like that observed at the narrows of the Baraboo river. The quartzite is of a lightish grey color, very compact and uniform, showing neither the tendency to pass into a sandstone, nor the deep red color apparent in some parts of the Sauk quartzite. The laminated structure is visible in some places, but never with the distinctness of that apparent at Devil's Lake. The dip is very obscure. The different systems of vertical joints—there seem to be as many as three—and the bedding joints, are so confused that it becomes very difficult without long study to ascertain the dip accurately. It certainly is at a very high angle. Considering together the directions of the various joints, the laminated structure, and the mode of occurrence of the intercalated talcose material, the weight of testimony would seem to point to a uniform N.N.E. dip, at a very high angle. Along the lines of separation of some of the joints —probably not bedding joints—the quartzite shows smooth polished faces of considerable extent; this peculiarity was observed with the Sauk quartzite also. Glacial markings are quite apparent in many places along the top of the ridge. The markings bear N.N.E. and S.S.W. The peculiar rounded form of exposed surfaces along the top would seem to be partly due to glacial action.

I was not able to find the quartzite and associated unaltered rocks in direct contact, but by carefully following the ridge to the southward, in which direction it gradually becomes lower, I was able to trace the outcrop of the quartzite continuously to within a few rods of a deep cutting on the Milwaukee and St. Paul railroad. In the lower part of this cut I found the horizontal layers of thinly laminated siliceous limestone, which are characteristic of the gradation of the Potsdam sandstone into the Lower Magnesian limestone. The occurrence of Blue or Trenton limestone not more than a mile to the westward, as represented on Dr. Lapham's map, was verified.

It would seem that three different theories have been advanced with regard to the age of these rocks: 1st, that they are Azoic or "primary;" 2d, that they are altered from the Potsdam sandstone; 3d, that they are altered from the St. Peter's sandstone. I make the following arguments in favor of the first of these.

1st, *The exceedingly limited area of disturbance.* This argument is the same as that advanced in favor of the Azoic age of the Sauk quartzites. The idea of such thorough alteration and disturbance over a small area, away altogether from any great system of metamorphism is inadmissible.

2d, *The occurrence close by of horizontal layers of Potsdam age.* These layers indicate the lower portion of the upper member of the Potsdam group. The quartzite, if altered from the "Lower" *sandstone*, must have been made, upheaved, and worn down *in the interval between the close of the sandstone epoch and the beginning of that of the limestone.* But these formations everywhere throughout the State graduate imperceptibly into one another; in other words, *there was no interval.* That the quartzite cannot have been altered from the St. Peter's sandstone, as stated by Percival, is also shown by these horizontal layers. A sandstone could hardly be changed to quartzite, whilst the beds immediately underlying it are left unaltered and undisturbed.

3d, *The thoroughness of the change in the rocks.* This is an additional proof that the metamorphism must have been a part of some great system of changes and foldings, and not the result of an action restricted entirely to an area of a few square miles.

4th, *The probable uniform dip to the N.N.E.* The indication of a *uniform dip* at a very high angle, together with the absence of any sign of an anticlinal, is direct proof that the quartzites are older than the undisturbed beds of the Potsdam period, which lie near by. The time that elapsed after the deposition of the Potsdam beds, must have been long enough to cover the time of upheaval and metamorphism, as well as the time requisite to erode all traces of an anticlinal.

I may say then confidently that these rocks are older than the Potsdam; that they received their present form before the laying down of the Lower Silurian strata; and that we find in them simply another outlying island in the Potsdam seas.

Relation to the main Azoic body, and to the other detached Azoic masses of the State. The accompanying outline map of Wisconsin serves to show these relations. A, A, A, is the southern line of the Azoic body; 1, 2, 3, 4, 5, the patches of quartzite, according to Lapham's map, lying within the Lower Silurian area; 6, 7, 8, 9, 10, are similarly isolated masses of granite or granitoid rocks. The Sauk quartzite ridges are marked 4; that of Portland 5. It will thus be seen that the last named is much the most distant from the Azoic body.

Can these quartzite areas be regarded as Huronian? The kinds of rocks (quartzites, conglomerates, siliceous slates), the distinct stratification, the no less distinct lamination, ripple markings, etc. (Devil's Lake), and the absence of granitoid rocks, would seem to show a close similarity between the rocks of these isolated areas and those in northern Wisconsin and northern Michigan now regarded as Huronian.

University of Wisconsin, Dec. 14, 1872.

ART. XXXII.—*On the Oregon Borate of Lime* (*Cryptomorphite?*); by A. W. CHASE.

CURRY County, Oregon, is the southernmost of the coast counties of that State, and lies directly north of latitude 42°, the boundary line of California. The entire county, with the exception of a narrow strip of arable land on the sea coast, and the alluvial bottoms of the Rogue and other rivers, is filled with ranges of rugged mountains, lateral spurs of the Siskyion, whose snow summits are everywhere visible. The only wheel-road in the county runs from the boundary to the Chetko river, a distance of eight miles; from thence a bridle trail leads to the settlements of Ellensburg, Port Orford, etc. Five miles north of the Chetko and directly on the sea coast is the locality of the borate deposit to be described. The country in the immediate vicinity resembles a succession of terraces, from one to two hundred feet above the sea level. From these terraces numerous rounded hills, some of them perfect cones, arise, generally reaching an altitude of two hundred feet above the flat. Back of these terraces lies a long range of mountains 2000 feet high. One peak of this range called Red Mountain, from the character of the rock exposed (a reddish sandstone), forms a perfect dome, and is at the head of the stream on which the borate was found, and five miles from the sea.

Through the terraces the brooks coming from this range have cut deep cañons, the rocks exposed in the beds being talcose slate and serpentine. Immediately in the vicinity of the borate deposit the hills slope downward so as to form three sides of a perfect crater, the bottom of which must be under water.

The coast is bordered with innumerable rocks of conglomerate and metamorphic sandstone, and the depth of water is very great within a few hundred feet of the shore line. The beach is strewn with masses of conglomerate rock composed of pebbles of agate, carnelian, jasper, and quartz, bound together by a cement of sandstone; through fissures and breaks in these rocks, veins of carbonate of lime of a milk-white color are found, many of them several inches in thickness. Two small streams cut their way down from the mountains and empty into the sea within the limits of the little bay, which is about three quarters of a mile in extent. On one of these streams a farmer located some ten or twelve years ago, and has occupied the place ever since, engaged in cattle raising. The locality was appropriately named the "Lone Ranch," his nearest neighbor being five miles distant. His attention was early called to an outcrop on the banks of the little stream, about 500 yards from the sea, and

20 feet above it, of a white substance which he called "chalk." When it was known that farmer Cresswell had chalk on his place, the coopers at the fisheries on the Rogue river and the carpenters in the little towns sent for some, and for years afterward it was used in cooperage and to chalk carpenters' lines. Masses of it exposed by winter floods were washed out to sea.

My attention was called to the deposit in 1872, and then chiefly from the fact that it proved an excellent polishing material for silver. On presentation of a specimen to the California Academy of Science by the President, Prof. Davidson, it was pronounced borate of lime. On a subsequent examination by the San Francisco Microscopical Society, Mr. H. G. Hanks, chemist, exhibited the rhombic character of the crystals, and also the fact that it contained no diatoms, and pronounced it cryptomorphite.

The substance having commercial value, it was mined during the past summer, and the following information concerning the character of the deposit obtained. In cutting away the bank above the spot where the first outcrop appeared, several layers or strata were met with, of varying thickness. The first in order was a soft green clay or decomposed talc, with streaks and globules of a white, waxy substance, itself another form of steatite. The second, a layer of black slate or rock broken into a thousand fragments and resembling coal. Then a layer of slate, seamed with white veins and masses of decomposed talc. These layers did not contain any traces of boric acid. Then came a layer of hard, white borate, filling the seams and cavities of the slate, and pressing down on the layer beneath, which was a tough blue steatite with green and white veins and of the consistency of clay. Wherever a hollow had formed in the blue steatite, the hard borate pressed down into it and formed a hemisphere, the upper parts being mixed with slate, the lower pure. In the blue steatite and a few inches below the slates, the main vein or flow was found. Here the borate was in the form of boulders or rounded masses, completely imbedded in the steatite, and in shape not unlike a pumpkin or squash, the sides being corrugated and having little depressions in the top surface. These boulders formed a continuous line touching each other, and were of uniform size in the main flow, weighing about two hundred pounds each, although some were much larger, one weighing four hundred and fifty. Branching off from the main deposit were side flows, where the boulders ran from twenty pounds down to small pellets the size of a pea, and even smaller.

These masses were all perfectly pure and each complete in itself. When broken apart the fracture exhibits no luster.

›olor is milk-white; feel greasy and unctuous. Before the ›ipe the substance exhibits the usual green flame.

e following is the analysis of the two kinds, the hard ǝ found in veins and the boulders or masses:

Boulders.

Water	22·75
Lime	29·96
Alkalies	·25
Chlorides	*traces*
Boracic acid	47·04=100·00

Hard Borate.

Water	25·00
Lime	29·80
Alkalies	*traces*
Boracic acid	45·20=100·00

e above analysis was made on average samples from large tities for commercial purposes, and owing to the presence large percentage of water the average of acid was reduced. ıall portion, thoroughly dried, previously tested, yielded as as 55 per cent. The quantitative analysis was made in ffice of Gen. Hewston, San Francisco Assay and Refining ks, by Mr. Thomas Price, chemist.

will be seen from the above analysis that soda is entirely ıt, the composition being purely lime, water, and boric forming thus a hydrous borate of lime. The appearance e material and the shape in which it is found does not spond with any of the described varieties in Dana's "Mingy," and it might be called a new variety. The adjacent ite and earths, as well as the slates, do not show the slightrace of boric acid. In working, empty cells were fretly struck, of the exact shape and size of boulders lying them. These cells had an inside incrustation of a semiparent nature, covered with little protuberances or pim- This substance also gave a green flame with sulphuric and alcohol.

forming a theory as to the origin of this deposit, it is ıssible to resist the conclusion that it came from a spring ›ric acid in the crater of what was probably a mud volcano. ıe different layers of steatite and clays being first ejected, ıcid, possibly in the form of vapor from a hot spring, must passed through a layer of carbonate of lime; holding this spension the vapor penetrated the possibly liquid mud, formed cells in it somewhat similar to air bubbles. As nud cooled it pressed on these cells from all sides—the ng of the vapor or water also allowed it to deposit; the er in suspension which pressed by the mud assumed the ded and corrugated shape that it is found in at present.

The vapors that escaped from the steatite were caught by the slate above, and formed as vein matter in the fissures.

Further development of this probably unique deposit may lead to a change in this theory, but at present it seems the only hypothesis to account for the presence of this pure borate in a substance which does not itself possess any trace of either the acid or lime.

ART. XXXIII.—*Explorations West of the* 100*th Meridian.*

(Communicated by Dr. H. C. YARROW.)

THE third field season of explorations and surveys under Lieut. Geo. M. Wheeler, Corps of Engineers, was brought to a close about the beginning of December last, and the scientific corps is now busily engaged in office-work in Washington, elaborating the data obtained. The areas embraced in the season's work covered western and southern Utah, eastern Nevada and northern Arizona, as far as the Grand Cañon of the Colorado, and taken in connection with the labors of 1869 and 1871, amount in extent to the territory of the New England and Middle States combined. These three surveys, in 1869, '70 and '72, have been made to supplement and perfect each other in such a way that the Lieutenant's mapped field now extends from central California over a large part of Nevada, as far east as central Utah, and south over the larger part of northern, western and central Arizona.

The initial aim in the work of the Survey is the accurate mapping of the countries traversed, and the correction of the engineers' map of the United States west of the 100th meridian. For this purpose there is a corps of trained topographers connected with the expedition, whose operations during the past season were much facilitated, and the accuracy of their results much enhanced, by means of a comprehensive series of astronomical stations, at various points, either nearly or remotely connected with the field of survey, extending from Cheyenne, Wyoming, on the line of the Union Pacific, to Beaver, in lower Utah. The value of these astronomical stations, and the tables of observations taken, can hardly be overestimated, when considered with reference to the perfecting of surveys in a country where so many physical obstacles stand in the way of accurate geodetic work. In addition to the astronomical work, hourly meteorological observations were made, at different points in the region surveyed, and the accumulation of data from this source alone is great. The various departments of geology, mineralogy, meteorology, natural history, and ethnology were filled and administered with vigor, and a valuable gallery of photographic views secured from the Grand Cañon of the Colorado, as well as from Utah, by Mr. Wm. Bell, a skilled photographer of Philadelphia; characteristic sets of views selected from these are now preparing, under orders from the War Department, for exhibition at the Vienna Exposition during the coming summer.

In the line of natural history, Dr. H. C. Yarrow and his assistant, Mr. H. W. Henshaw, made valuable collections from the flora and fauna of the territory entered, including not less than five hundred bird-skins from Utah, which have been received in Washington in excellent preservation. The geologists of the expedition, Messrs. G. K. Gilbert and E. E. Howell, besides the strictly special work of their profession, effected a labor of economic value, by entering many mining camps in Utah and Nevada, and examining thoroughly the developments and possibilities of the various districts; aided in this by the surveyors and astronomers of the corps, who set up, in several mining districts, astronomical monuments bearing the latitude and longitude of the places, for the convenience of local surveys. The topographers of last season were Messrs. Louis Nell, J. Weyss, H. Crueger, and G. Thompson, and the astronomers, Messrs. J. H. Clark, E. P. Austin and W. W. Maryatt. The ethnologist of the expedition, Mr. M. S. Severance, made extensive researches in the ancient mounds, and recent graves, of the Indian country, and secured from these sources a dozen or more crania and skeletons, besides numerous relics of various age and value, and a considerable addition to the Ute and other dialects. Lieuts. R. S. Hoxie and W. L. Marshall, of the engineers, aided Lieut. Wheeler in the executive administration of the expedition, besides giving somewhat attention to astronomical and topographical subjects. Lieuts. W. A. Dinwiddie and W. Mott were in command of the cavalry and infantry escort, respectively.

Much of Lieutenant Wheeler's attention was devoted to subjects collaterally associated with the main object of the survey, relating to the internal economy of the vast areas traversed, and the elucidation of industrial questions of vital interest to settlers and miners; among these were the subjects of artesian wells and schemes of irrigation to reclaim the wide-spread barren wastes of that region, the limiting of the areas within which mineral croppings may reasonably be expected, the discovery of new routes of travel and transportation, and favorable positions for the establishment of military posts, the collection of data bearing on agricultural questions and the limits of population, and other investigations of equal importance in the development of a new country and the improvement of regions sparsely settled.

ART. XXXIV.—*On Footprints in the Carboniferous rocks of Western Pennsylvania*; by W. D. MOORE.

IN 1846, Dr. Isaac Lea discovered, in the coal strata near Pottsville, footprints similar to those discovered by Dr. King in 1844. Another example of similar footprints has been discovered in a quarry, on the farm of Commodore Rogers, three miles north of the city of Pittsburgh, Pennsylvania. The tracks found by Dr. King occurred about 100 to 120 feet below the Pittsburgh coal seam, near a little town called Pleasant Unity, not far from the base of Chestnut ridge. In precisely the same situation geologically were the tracks found of which I send you a cast, and rather imperfect drawing. The Pittsburgh coal seam is worked on the hill side, above the quarry from which these tracks were obtained, by actual measurement 110 feet.

As in the tracks described by Dr. King, one of the toes stands out like an opposable thumb, giving the appearance of a hand to the track, and the number of toes is greater in the hind than the fore foot. The front track shows but three toes, but the abnormal thickness of the third toe shows to me conclusively that there were two, which have been, through a filling up with the soft mud, warped together; and the same has very nearly happened with the third and fourth toes of the hind foot. I believe also that a depression to the right of the heel of the front track, and to the left of the heel of the hind track, indicates the place of the thumb-like toe, which has been in both cases washed away; the very noticeable hollow running round both feet indicates the channeling out and washing away here suggested. If I am right in this supposition, then the tracks must have been originally at least a third larger. In giving the following description, however, I state the characters just as they are on the slab, referring the tracks provisionally to the genus *Cheirotherium*.

Cheirotherium Reiteri: Toes of hind foot four; directed to the left; left three of nearly equal length, 1¼ inches; the fourth much smaller and shorter, ¾ inch; heel 1 inch long; 1½ inch wide; length of foot 2¼ inches to 3 inches; length of step, 11 inches. Toes of the front foot three, directed to the right, of nearly equal length, thicker and shorter than those of the hind foot; heel apparently longer and wider.

The species is dedicated to Dr. Wm. C. Reiter, of Pittsburgh, an accomplished naturalist and an intimate friend and fellow laborer, in scientific pursuits, of Dr. King, the first discoverer of reptilian tracks in the Coal measures of Pennsylvania. The specimen is in the collection of the Western University of

Pennsylvania, and a second cast is in possession of Dr. W. C. Reiter, of Pittsburgh, Pa.

Before closing, I may remark that the sandstone, on which these tracks are found, like that, on which Dr. King's were found, is covered with mud-cracks, and numberless long trails, as of animals passing over the surface, but to what class referable I cannot tell. It is, moreover, covered with the most beautiful ripple marks; in one place in the quarry, a space of the stone about thirty yards long and twenty wide is exposed to view, and could easily at a little distance be mistaken for a shore just lapped by the retiring waves. In one locality, at the same geological horizon, Westmoreland county, Pa., some years since, I found an extensive and remarkable deposit of fossils, indicative of the conditions supposed to exist where these and similar tracks are found. In the thin black shales of that locality were confusedly mingled the characteristic plants of the coal and marine shells, a singular new species, described by my friend Leo Lesquereux as "*Crematopteris Pennsylvanica*"* and a well marked wing of an insect closely allied to that since found by him in the low coal of Arkansas, and described as "*Blattina venusta.*"† Some years subsequently at nearly the same geological horizon, in Washington county, Pa., I found a mass of fossil remains wholly unknown to me, but which I supposed to be fragments of a reptilian skeleton. They were given by me to Prof. Leo Lesquereux, and by him forwarded to Prof. Agassiz. I have not learned the result of Prof. Agassiz's examination.

ART. XXXV.—*Additional Observations on the Dinocerata*; by O. C. MARSH.

IN the article on Gigantic Fossil Mammals in the February number of this Journal (p. 117), I called attention to numerous errors in Prof. Cope's recent publications on the same subject. Some of these mistakes were made by Prof. Cope in describing his own specimens; some by misunderstanding the characters of allied *Dinocerata;* and not a few in giving dates and references which were not correct. These specific points against his work, some twenty in number, Prof. Cope has not answered. He has, however, endeavored to break the force of my criticism by a general denial, which evades the main issue between us, as I have recently shown in the American Naturalist for March (p. 146). He says, in substance, that one species of *Eobasileus* is different from one of my species; but this would not materially diminish the list of his errors on this subject. Prof. Cope distinctly included in his group of supposed Proboscidians the genera *Dinoceras* and *Uintatherium*, thus mistaking, as I have shown, both their characters and affinities, as well as those of his own specimens. Prof. Cope states further, that I have not seen his *Eobasileus*. I have already expressed my belief that what he has called *Eobasileus* was merely my genus *Tinoceras*, and, as shown below, this is now established beyond a doubt. Prof. Cope asserts, likewise, that the descriptions he has given are correct; but this is impossible, since he has made most contradictory statements about the same characters of the same specimen. Prof. Cope has said, e. g., that *Eobasileus* had upper incisors, but no canines; next, that it had canines, but no incisors; and, finally, that it had one incisor and one canine. He has stated, also, that the anterior horn-cores were on the frontal bones; that they were on the nasals; and even that they were in part on the maxillaries. He tells us, moreover, that the nasal bones were greatly elongated; and again, that they were extremely short; that the horn-cores of *Eobasileus* were six inches in length; and, next, that they were a foot long; that the spine of the tibia was obtuse; and again, that it was wanting; and, strangest of all, that the premaxillary was both edentulous, and a trenchant tusk!

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* Geological Report of Pennsylvania, vol. ii, p. 868.

† Geological Reconnaissance of Arkansas, p. 314.

The March number of the Naturalist, in which I have pointed out Prof. Cope's numerous errors on this subject, contains another article by him on the *Dinocerata*. This paper, likewise, is not free from serious mistakes and inaccuracies, which show that Prof. Cope still misinterprets some of the most important characters of his own specimens. The paper purports to have been read at the Dubuque meeting of the American Association of Science, but it evidently includes the results of Prof. Cope's later investigations, as well as corrections suggested by my recent criticisms. This is equally the case with the appended paper, which was first issued separately, and has just been re-published in an emended form.*

Since the March Naturalist was published, I have had an opportunity, through the kindness of Prof. Agassiz, of examining a series of photographs of the skull described as *Eobasileus cornutus*. These views fully confirm my previous belief in regard to this specimen, viz: that it belongs to the genus *Tinoceras*, and hence to the *Dinocerata*, the species being apparently *T. grandis* Marsh. The photographs show, moreover, that Prof. Cope has from the first mistaken many characters of this genus, and hence his erroneous conclusions in regard to the group to which it belongs.

Some of Prof. Cope's errors in dates and references which I have pointed out, he has corrected in his later papers, and these need not be repeated. In his last note on *Eobasileus*,† however, he has re-asserted that his descriptions are correct, and hence this point deserves consideration, especially as we now have the means of testing the accuracy of these descriptions. The photographs of Prof. Cope's *Eobasileus*, examined in connection with many similar remains in the Yale Museum, make it evident that the various objections I have raised against his work are, almost without exception, well founded. His more recent errors, as well as those remaining in his dates and references, should be placed in the same list, in judging of the value of his papers on the *Dinocerata*. These papers still need correction on the following points:—

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* Proceedings Philadelphia Academy, p. 11, 1873.

† American Naturalist, vii, p. 180, March, 1873.

1st. The name *Eobasileus* Cope is a synonym of *Tinoceras* Marsh, which antedates it (p. 122), and the name of the family, *Tinoceratidæ*, likewise has priority over *Eobasiliidæ*. 2d. The name *Loxolophodon* Cope should not be applied to this genus, as there is no satisfactory evidence that the single premolar tooth to which it was first given is generically identical, and the probabilities are against it. 3d. The species *Eobasileus cornutus* Cope appears to be the same as *Tinoceras grandis* Marsh, which was first described (Sept. 21, 1872). The species *E. furcatus* Cope, founded on portions of supposed nasal bones (which Prof. Cope has since called frontal bones), has at present no authority, the specimens described being evidently the posterior horn-cores of other known species. Judging from the descriptions, the name *E. pressicornis* Cope has apparently no better foundation. 4th. The genus *Dinoceras* Marsh is distinct from *Uintatherium* Leidy, although perhaps nearly related. 5th. The mammals of the above genera, and probably those of *Megacerops* Leidy, cannot be placed in the order *Proboscidea*, but constitute a distinct group, *Dinocerata*, which approaches the perissodactyls rather than the elephants. 6th. The presence of a proboscis does not directly result from the osteological characters of this group, but is quite inconsistent with them, and the evidence is decidedly against it. 7th. The skull in the *Dinocerata* has no distinctive proboscidian features, and the subordinate similarity in the limb-bones I pointed out before Prof. Cope wrote anything on the subject. 8th. The presence of canine teeth and horns was not alone stated by me to be characteristic of a new order, but other important characters were given (p. 121). 9th. The *Dinocerata* have no upper incisors, and the teeth called incisors by Prof. Cope are canines. 10th. These canines do not correspond to the tusks of Elephants, and the latter are not enclosed between the premaxillary and maxillary, but are inserted in the former bone. 11th. The nasal bones in the *Dinocerata* are much elongated, and their free extremities are not extremely short, or deeply excavated. 12. The frontal bones do not extend in front of the premaxillaries; their extremities do not form bony projections like shovels; and they do not support horns or processes at both extremities. 13th. The anterior horn-cores are on the nasal bones, and not on the frontals; and they are not composed externally of the maxillaries. 14th. The middle pair of horn-cores, likewise, are not on the frontals, but on the maxillaries, their inner inferior margin alone being formed of the nasals. 15th. The orbits were not below these horns, but behind them, especially when the head was in its natural, declined position. 16th. The malar does not form the middle of the zygomatic arch, merely, as in the Elephant, but extends in front of the orbit, as in the Tapir. 17th. The temporal fossæ are not small posteriorly. 18th. The occiput is not vertical, but oblique. 19th. The tarsus and foot are not proboscidian in character, strictly, but show strong perissodactyl features, e. g., in the absence of a hallux, and in the articulation of the astragalus with both the navicular and cuboid bones. 20th. The genus *Dinoceras* was not originally referred to the Perissodactyls, but to a new order. 21st. The name *Tinoceras* was not first proposed August 24, 1872, but August 19, 1872, and on that day I mailed Prof. Cope the pamphlet containing it. 22d. The characters given in describing *Tinoceras anceps* Marsh* pointed directly toward the Proboscidians, and this fact was distinctly stated. 23d. Many of the erroneous dates and references which I have pointed out (pp. 118, 122, and 135) in Prof. Cope's recent publications remain uncorrected.

The species of *Dinocerata* at present known with certainty are the following:—*Tinoceras anceps* Marsh, *Tinoceras grandis* Marsh, *Uintatherium robustum* Leidy, *Dinoceras mirabilis* Marsh, *Dinoceras lacustris* Marsh. To these should probably be added *Megacerops Coloradensis* Leidy, and also *Tinoceras cornutus* = *Eobasileus cornutus* Cope, if this species should eventually prove distinct.

Yale College, New Haven, March 10, 1873.

SCIENTIFIC INTELLIGENCE.

I. CHEMISTRY AND PHYSICS.

1. *Considerations on some Points of the Theoretic Teaching of Chemistry.*—The Faraday lecture before the Chemical Society of London, was delivered by Professor CANNIZZARO, now of Rome, upon the above subject. Standing among the first of living chemists, as the distinguished author does, his views upon the value of chemical theories in teaching have an importance amply justifying some notice of them here. Accepting fully the theory of atoms and molecules, and believing that "this theory affords the clearest, shortest, most exact and most accessible summary of all that relates to the origin, meaning, value and use of empirical formulæ and of equations," he naturally concludes that "it ought to be introduced into the teaching of chemistry at an early stage." "I do not hesitate to assert," he continues, "that the theory of atoms and molecules ought to play in the teaching of chemistry, a part analogous to that of the theory of vibrations in the teaching of optics." Affirming that "the solid base, the corner stone of the modern theory of molecules and atoms, is the theory of Avogadro, Ampère, Krönig and Clausius on the constitution of perfect gases," he would "found on this theory the demonstration

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* This Journal, vol. ii, p. 35, 1871.

of the limits of divisibility of elementary bodies; that is to say, the existence of elementary atoms." With his own classes, Cannizzaro begins by establishing the invariability of mass in chemical changes, pointing out the fact that "the only constant property of matter is its ponderability." He then passes to the Daltonian theory, establishes, by means of Gay Lussac's law of combining volumes, the correctness of the atomic weights, and then easily demonstrates the molecular condition of simple matter; thus placing the fundamental notions of atom and molecule upon a solid basis, freed from everything not necessarily connected with them. Then only it is "that we are in a position to attack the difficulties encountered in the applications of these notions to particular cases." The assistance of specific heat, of isomorphism and of chemical analogy in fixing the size of molecules, and the true meaning and value of the theory of atomicity may then be introduced, the student being taught to recognize the dynamic as well as the ponderable phenomena of chemical change.—*J. Chem. Soc.*, II, x, 941, Nov., 1872. G. F. B.

2. *On the Relation between Vibration and Detonation.*—CHAMPION and PELLET have made some experiments which fully confirm Abel's theory that detonants explode by a peculiar mechanical vibratory stimulus. They found, for example, that nitrogen iodide could be exploded at one end of a glass tube 22 feet long by means of a little of the same substance detonated at the other; and this when the tube was in two pieces joined in the center by a paper band. On fastening a bit of nitrogen iodide by means of gold-beater's skin, to the strings of a bass-viol, it was observed that when placed on the two lower strings it did not explode when these were vibrated; while that on the highest string exploded instantly. This string vibrated 30 times per second. Similar results were obtained with Chinese gongs. To show that heat had nothing to do with the result, the quantity of nitro-glycerin, of mercuric fulminate, and of gunpowder necessary, when placed in one focus of a parabolic mirror, to explode nitrogen iodide at the other, was measured; and it was found that an amount of gunpowder was needed which would evolve ten times as much heat as the necessary amount of nitro-glycerin. Moreover, blackening the mirrors prevented the explosion by gunpowder, but left that by nitro-glycerin unaffected. The authors subsequently reversed the experiments and studied the vibrations produced by the detonation of the above compounds. By means of a series of sensitive flames, arranged according to the complete scale of *g* major, and before which at five meters distance, 0·03 grm. nitrogen iodide or mercuric fulminate was exploded, they showed (1) that the vibrations caused by the two explosions are very different, the former having no effect, while the latter excited the flames *a*, *c*, *e*, *f*, *g*; and (2) that the vibrations produced belong only to certain notes of the scale. Nitro-glycerin, nitro-glycol, nitro-erythrite and nitro-dulcite, gave no results with this apparatus.—*C. R.*, lxxv, 110 and 712, 1872. G. F. B.

3. *On Cœrulignone, a coloring matter from Pyroligneous acid.*—When the crude calcium acetate obtained in the distillation of wood is distilled with the necessary quantity of hydrochloric acid, and the distillate is purified by adding some potassium dichromate, there forms a blue film upon the surface of the acetic acid, which finally sinks and forms a violet sediment. LIEBERMANN has examined this substance. After washing, he dissolved it in cold phenol and precipitated it by alcohol in the form of dark steel blue needles, which had the composition $C_{15}H_{14}O_6$ or $C_{30}H_{30}O_{12}$, and to which the author gives the name *cœrulignone*. It is almost insoluble in ordinary solvents, concentrated sulphuric acid dissolving it with a blue color, potassium hydrate to a green liquid, while nitric acid oxidizes it to oxalic acid. It is decomposed by heat and does not dye either with or without a mordant. Observing its formation by the addition of potassium dichromate to several samples of crude acetic acid, Liebermann inferred the presence in the acid of some body which yielded it on oxidation. Hence he reduced cœrulignone by tin and hydrochloric acid, and obtained a colorless body in crystalline needles, $C_{15}H_{16}O_6$, soluble in alcohol, melting at 190°, capable of being distilled, and changing to cœrulignone by oxidation. He calls it *hydro-cœrulignone*, and considers it a phenol, of which cœrulignone is its quinone or its quinhydrone.—*Ber. Berl. Chem. Ges.*, v, 746, Oct., 1872. G. F. B.

4. *On the Synthesis of Anthracene.*—Limpricht produced anthracene by heating benzyl chloride with water. Beside anthracene $C_{14}H_{10}$, there is also produced a liquid hydrocarbon $C_{14}H_{14}$. VAN DORP, having observed in the synthesis of dimethylanthracene, the production of a homologue of this body, which, passed through a red hot tube, was converted into dimethylanthracene with evolution of hydrogen, was led to test the same question in the case of the hydrocarbon $C_{14}H_{14}$. By Limpricht's method he prepared this substance, boiling at 280°, and found that by passing it through a red hot tube filled with fragments of pumice, hydrogen was evolved and anthracene abundantly formed. As this liquid hydrocarbon, when oxidized with chromic acid, yielded benzoylbenzoic acid, it must consist of benzyltoluol, $C_6H_5CH_2\text{--}C_6H_4CH_3$. To prove it, benzyltoluol, boiling constantly at 275° to 280°, was prepared and its vapor conducted through tubes containing pumice fragments and heated to low redness. Pure hydrogen was evolved and there condensed in the tube a solid hydrocarbon mixed with the excess of liquid used. On expression and repeated crystallization from glacial acetic acid, slightly yellow crystalline plates, fusing at 213° and having the composition $C_{14}H_{10}$ were obtained. Conversion into picrate, and the production of anthraquinone and alizarin proved it to be anthracene. The yield was but 10 per cent. Yet by improved methods it may be increased probably to the theoretical amount. The author believes the process to be of technical value. —*Ber. Berl. Chem. Ges.*, v, 1070, Jan., 1873. G. F. B.

5. ***New method for preparing Alizarin.***—GIRARD proposes the following method for the preparation of alizarin from crude anthracene, boiling between 300° and 335°: Treat the mass first with potassium chlorate and hydrochloric acid, in order to obtain tetrachlorinated products. Oxidize these, either by nitric acid or by a metallic oxide (as lead peroxide), with sulphuric or acetic acids; obtaining thus a mixture of dichloranthraquinone and chloroxyanthranil chloride. In presence of a metallic oxide (zinc, copper or lead oxide) treat these bodies with an alcoholic solution of sodium acetate. The last atom of chlorine is thus removed and alizarin is the result. This may then be purified.—*Bull. Soc. Ind. Mulhouse*, xlii, 54; *J. Chem. Soc.*, II, x, 1138, Dec., 1872.

G. F. B.

6. *On the Hydrates of Monobasic fatty acids.*—GRIMAUX suggests the existence of bodies analogous to the glycerins, but in which the hydroxyls are united to the same carbon atom. These bodies he calls *carberins*. They are produced by hydrating the monobasic fatty acids. Thus, fomic acid $CH\left\{\begin{matrix}O\\OH\end{matrix}\right.$ plus H_2O gives $CH\left\{\begin{matrix}OH\\OH\\OH\end{matrix}\right.$ formyl-carberin; acetic acid $CH_3.C\left\{\begin{matrix}O\\OH,\end{matrix}\right.$ gives $CH_3.C\left\{\begin{matrix}OH\\OH\\OH,\end{matrix}\right.$ acetyl-carberin. These hydrates are known and have been described. Moreover, chloral hydrate is a chlorinated glycol, having two hydroxyls united to the same C atom; $CCl_3\text{-.-}CH\left\{\begin{matrix}OH.\\OH\end{matrix}\right.$ The carberin ethers, as $CH\left\{\begin{matrix}(CHO_2)\\OH\\OH\end{matrix}\right.$ or $CH_3.C\left\{\begin{matrix}(C_2H_3O_2)\\OH\\OH,\end{matrix}\right.$ are diformic or diacetic acids; and $CH_3.C\left\{\begin{matrix}(C_4H_7O_2)\\OH\\OH\end{matrix}\right.$ is aceto-butyric acid. The chlorhydrin $CH\left\{\begin{matrix}Cl\\Cl\\Cl\end{matrix}\right.$ is chloroform; $CH\left\{\begin{matrix}OC_2H_5\\OC_2H_5\\OC_2H_5\end{matrix}\right.$ is known as ethyl subformate.—*Bull. Soc. Ch.*, II, xviii, 535, Dec., 1872.

G. F. B.

7. *On a Boiler Incrustation from New Jersey;* by GEORGE A. KOENIG, Ph.D.—Some time ago Mr. Joseph Harrison, Jr., presented to the Academy a specimen of boiler incrustation from Orange Co., N. J. The physical properties of this incrustation were remarkable enough to suggest a chemical examination. It was about half an inch thick, presented a smooth surface, was hard and coherent, of a brownish flesh-color, and showed on the fracture a distinct prismatic structure, the prisms standing vertically on the surface. It looked very much like the so-called "Sprudelstein" fron Karlsbad in Bohemia, which is aragonite.

The analysis gave the following results:—Sulphuric acid (SO_3) 57·58, calcic oxide (CaO) 40·40, ferric oxide (Fe_2O_3) 0·54, silicic acid (SiO_2) 0·05, organic substance and water 1·00=99·57.

57·58 parts of sulphuric acid require, by theory, 40·306 parts of calcic oxide to form calcic sulphate, which latter number corresponds perfectly with the one found; we can say, hence, that the incrustation is composed of: Calcic sulphate 97·89, ferric hydrate 0·72, silica 0·05, organic matter 0·82.

To my knowledge there has not been described, so far, a boiler incrustation which is so very near chemically pure calcic sulphate, and none in which this is so perfectly anhydrous. We know that calcic sulphate occurs in nature in two forms. In one it is combined with two equivalents of water, viz:—Calcic sulphate 79·07, water 20·93, crystallizing in oblique rhombic prisms, and is called gypsum. In the other it is without water, crystallizes in orthorhombic prisms, and is named anhydrite.

We know, further, that gypsum begins to lose water not much above the boiling point of water, and can be rendered anhydrous by prolonged heating at about 200° Centigrade. Still it seemed of interest to ascertain what changes would take place in a saturated solution of gypsum when evaporated under the atmospheric pressure at the boiling point, and also under a higher pressure.

A saturated solution of gypsum was kept at the boiling point until an ample amount of precipitate had formed. This precipitate consisted of minute scales with a marked silky luster. Under a magnifying power of 60 diameters the scales proved to possess the characteristic tabular forms of *gypsum* with the oblique base. They were perfectly transparent, and many were twins, those swallow-tail-shaped forms so well known. Upon ignition 20·7 per cent of water was found instead of 20·93, as required by the formula.

A saturated solution of gypsum was now sealed up in a glass tube, and kept in an oil bath for fourteen hours at a temperature of 148° Centigrade. This temperature is equal to a pressure of 4·4 atmospheres, or 66 pounds to the square inch. A slight granular precipitate was found on the glass after removing the tube from the bath and drawing off the mother liquor. Under a magnifying power of 120 diameters the apparent granules dissolved into stellate groups of needle-shaped crystals intermixed with single needles of a larger size. Most of the crystals had the oblique rhombic basal termination of gypsum; but some showed an orthorhombic basis. All the crystals had became opaque, apparently by innumerable fissures, as a network could be discerned in many individuals. The presence of prismatic protuberances on some of the crystals standing at right angles to the principal axis of the main crystal seemed very curious.

The precipitate was now removed from the tube and carefully washed, then dried over sulphuric acid. After ignition a loss was produced of 3·1 per cent. Taking into consideration that not all of the mother liquor was washed out and crystallized as gypsum, this result may be looked upon as confirmatory that the whole of the precipitate is *anhydrite.* Professor Genth is of opinion that the opaque stellate crystals are pseudomorphs of anhydrite after gypsum, and I see at present no reason to the contrary.

In nature we find the anhydrite associated with rock salt. Supposing that the deposition of the chlorides of sodium and potassium took place under a moderately high column of saturated water, the pressure exercised by this column would give a satisfactory explanation for the fact that calcic sulphate crystallized as anhydrite. The presence of gypsum in the same deposits would suggest a subsequent metamorphosis of the anhydrite into gypsum by taking up water.—*Proc. Acad. Nat. Sci. Philad.*, Jan. 4.

8. *Effect of Light on Selenium during the passage of an electric current.**—Being desirous of obtaining a more suitable high resistance for use at the Shore Station in connection with my system of testing and signalling during the submersion of long submarine cables, I was induced to experiment with bars of selenium, a known metal of very high resistance. I obtained several bars varying in length from 5 to 10 centimeters, and of a diameter from 1 to 1½ millimeters. Each bar was hermetically sealed in a glass tube, and a platinum wire projected from each end for the purpose of connection.

The early experiments did not place the selenium in a very favorable light for the purpose required; for, although the resistance was all that could be desired—some of the bars giving 1,400 megs. absolute—yet there was a great discrepancy in the tests, and seldom did different operators obtain the same results. While investigating the cause of so great differences in the resistance of the bars, it was found that the resistance altered materially according to the intensity of light to which it was subjected. When the bars were fixed in a box with a sliding cover, so as to exclude all light, their resistance was at its highest, and remained very constant, fulfilling all the conditions necessary to my requirements; but immediately the cover of the box was removed, the conductivity increased from 15 to 100 per cent, according to the intensity of the light falling on the bar. Merely intercepting the light by passing the hand before an ordinary gas-burner placed several feet from the bar increased the resistance from 15 to 20 per cent. If the light be intercepted by rock salt or by glass of various colors, the resistance varies according to the amount of light passing through the glass.

To ensure that temperature was in no way affecting the experiments, one of the bars was placed in a trough of water so that there was about an inch of water for the light to pass through, but the results were the same; and when a strong light from the ignition of a narrow band of magnesium was held about nine inches above the water the resistance immediately fell more than two-thirds, returning to its normal condition immediately the light was extinguished.—*Nature*, Feb. 20.

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* Communicated to the Society of Telegraph Engineers, February 12, by Mr' Latimer Clark, from Mr. Willoughby Smith, Electrician to the Telegraph Construction Company.

II. GEOLOGY AND NATURAL HISTORY.

1. *Note on the History of certain recent views in Dynamical Geology;* by ROBERT MALLET, F.R.S. (Letter to the Editors, dated Feb. 27, 1873.)—In reference to the notices, which are found in the February number of the American Journal of Science, of my views as to the nature and origin of volcanic energy and heat, and to a note at p. 156, by Professor Jos. LeConte, will you allow me to state that no question of priority can arise, as it seems to me, between that gentlemen and myself, as evolving from his very able paper in your Journal of November, 1872.

Neither Professor LeConte nor myself have any claim to the general idea of the elevation of mountain chains, etc., by tangential or lateral pressure. That belongs to Constant Prevost, who distinctly enunciated the doctrine and many of its consequences nearly forty years ago, though, like many other great and pregnant truths, this was for a long time completely and is even yet much neglected.

My own claims to originality comprised in my paper, read in abstract only to the Royal Society of London, and I regret to say not even yet published in full by that body, and of which I have given some account in my introduction to the translation of Prof. Palmieri's Eruption of Vesuvius of 1872, may be principally as follows: 1st, That volcanic heat and energy have their origin in the *transformation* of work into heat, the work arising from the movements (chiefly of descent) of the crust of a terraqueous cooling planet.

2d. The *colligating* the phenomena: 1st, of deformation of this spheroid, producing ocean and land; 2d, of elevation of mountain, and generally of all elevation and *depression* of hypogeal origin, including fissures and faults, etc.; 3, of vulcanicity, including volcanoes and earthquakes, etc., as all successive results of the same simple cosmical mechanism, the energy of which has decayed and is decaying with time, since the period when the train commenced, viz., when our globe became a molten spheroid thinly crusting over.

Those views have for many years been gradually maturing in my thoughts. I have from time to time communicated them more or less fully to several scientific friends, especially to my friend Professor Houghton, of the University of Dublin. I also took date as to them by letter addressed to Professor Stokes, Secretary of the Royal Society of London, dated January, 1870, a copy of which is before me. So far therefore, as there may be anything in common beween the thought of the valuable and suggestive paper of Prof. LeConte and my own above referred to, I endorse his statement that they are independent respectively in date as in conception.

I have looked into Mr. Vose's work on Orographic Geology, published 1866, to which Prof. LeConte's note (p. 156) directs attention in these words:

In justice to Vose, I ought to draw attention to the fact, that et's very excellent idea that *heat is produced by pressure*, is ght out distinctly in his [Vose's] volume. He, however, did extend the idea to vulcanism but only to metamorphism."

ose's expressions are everywhere very vague; the clearest I can find occur at pp. 109–110 and 132, in which it is quite that he connects in some unexplained way, as contempora- s action, heat-pressure and chemical forces as conjointly ome way giving rise to metamorphism; but I am unable scern any evidence that he entertained the slightest notion e *transformation of work into heat*, as producing meta- hic or any other geological effects. On the contrary, Prof. onte's own words, *heat produced by pressure*, may describe e's notions, so far as I can gather them; and if I have failed o so or have misstated them I shall be happy to be corrected; inly they do not properly describe mine. Heat *cannot* be uced by pressure; pressure and motion, i. e., work, can be *sformed into heat*, but of such a transformation I fail to find Vose has given any hint.

The Classification of the Pleistocene Strata of Britain and Continent by means of the Mammalia; by W. Boyd Daw- , Esq., M.A., F.R.S., F.G.S.—The Pleistocene deposits may ivided into three groups:—1st, that in which the Pleistocene igrants lived, with some of the southern and Pliocene animals ritain, France, and Germany, and in which no arctic mam- a had arrived; 2d, that in which the characteristic Pliocene idæ had disappeared, and the *Elephas meridionalis* and *Rhi- ros Etruscus* had been driven south; 3d, that in which the arctic mammalia were the chief inhabitants.

e third or late Pleistocene division must be far older than any istoric deposits, as the latter often rest on the former, and omposed of different materials; but the difference offered by auna is the most striking. In the Pleistocene river-deposits ty-eight species have been found, the remains of man being iated with the lion, hippopotamus, mammoth, wolf, and rein- On examining the fauna from the ossiferous caves, we find ame group of animals, with the exception of the musk-sheep; it is therefore evident that the cave-fauna is identical with of the river strata, and must be referred to the same period. e few animals, however, which would naturally haunt caves, eculiar to them, as the cave-bear, wild cat, leopard, &c.

e magnitude of the break in time between the Prehistoric and Pleistocene period may be gathered also from the disappear- in the interval of no less than nineteen species.

e middle division of the Pleistocene mammalia, or that from h the Pliocene Cervidæ had disappeared, and been replaced vading temperate forms, is represented in great Britain by eposits of the Lower Brick-earths of the Thames valley, and lder deposits in Kent's Hole and Oreston. The discovery, he Rev. O. Fisher, of a flint-flake in the undisturbed Brick-earth at Crayford, proves that man must have been living at this time. The mammalia from these deposits are linked to the Pliocene by the *Rhinoceros megarhinus*, and to the late Pleistocene by the *Ovibos moschatus*. The presence of ***Machærodus latidens*** in Kent's Hole, and of the *Rh. megarhinus* in the cave of Oreston, tends to the conclusion that some of the caves in the south of England contain a fauna that was living before the late Pleistocene age. The whole assemblage of middle Pleistocene animals evinces a less severe climate than in the late Pleistocene times.

The fossil bones from the Forest-bed of Norfolk and Suffolk show that in the early Pleistocene mammalia there was a great mixture of Pleistocene and Pliocene species. It is probable also that the period was one of long duration; for in it we find two animals which are unknown on the continent, implying that the lapse of time was sufficiently great to allow of the evolution of forms of animal life hitherto unknown, and which disappeared before the middle and late Pleistocene stages.

The author criticised M. Lartet's classification of the Late Pleistocene or Quaternary period, by means of the cave-bear, mammoth, reindeer, and aurochs, and urged that, since the remains of all these animals were intimately associated in the caves of France, Germany, and Britain, and, so far as we know, the first two appeared and disappeared together, and the last two lived on into the Prehistoric age, they did not afford a basis for a chronology.

The latest of the three divisions of the British Pleistocene fauna is widely spread through France, Germany, and Russia, from the English Channel to the shores of the Mediterranean. The Middle Pleistocene is represented by a river-deposit in Auvergne, and by a cave in the Jura, in which the presence of the ***Machærodus latidens***, and a non-tichorine rhinoceros, and the absence of the characteristic arctic group of the late Pleistocene, and of all the peculiar animals of the early Forest-bed stage, prove that that era must be Middle Pleistocene. The Early Pleistocene division is represented in France by the river-deposit at Chartres, being characterized by the presence of two non-Pliocene animals, *Trogontherium* and *Cervus varnutorum*.

The Pleistocene mammalia of the regions south of the Alps and Pyrenees present no trace of truly arctic species, the mammoth being viewed as an animal fitted for the climatal conditions both of Northern Siberia and of the southern states of America. It contains *Elephas Africanus* and *Hyæna striata*.

The fauna of Sicily, Malta, and Crete differs considerably from that described above, possessing some peculiar forms, such as *Hippopotamus Pentlandi*, *Myoxus Melitensis* and *Elephas Melitensis*.

The Pleistocene mammalia may be divided into five groups, each marking a difference in the climate:—the first embracing those which now live in hot countries; the second those which inhabit northern regions, or high mountains, where the cold is severe; the third those which inhabit temperate regions; a fourth those which are found alike in hot and cold; and a fifth, which are extinct.

There were three climatal zones, marked by the varying range of the animals:—the northern, into which the southern forms never penetrated, the latitude of Yorkshire being the boundary of the advance of the southern animals; the southern, into which the northern species never passed, a line passing through the Alps and Pyrenees being the limit of the range of the northern animals; and an intermediate area, in which the two are found mingled together.

Two out of the three zones are proved by the physical evidence of the Pleistocene strata.

We see by the discoveries of Dr. Bryce, Mr. Jameson, and others, that the Pleistocene mammalia must have invaded Europe during the first Glacial period before the submergence; for the reindeer and the mammoth have been found in Scotland under the deposits of the Boulder-clay. Dr. Falconer and others have also discovered the latter animal in the Preglacial Forest bed. The Glacial period can therefore no longer be looked on as a hard and fast barrier separating one fauna from another. If man be treated as a Pleistocene animal, there is reason to believe that he formed one of the North Asiatic group, which was certainly in possession of Northern and Central Europe in Preglacial times.

The Pleistocene mammalia may again be divided into three groups—those which came from Northern and Central Asia, those from Africa, and those which were living in the same area in the Pliocene age. Had not the animals which lived in Europe, during the Pliocene age, been insulated from those which invaded Europe from Asia, by some impassable barrier, the latter would occur in our Pliocene strata as well as the former. Such a barrier is offered by the northern extension of the Caspian up the valley of the Obi to the Arctic sea. The animals of Northern and Central Asia could not pass westward until the barrier was removed by the elevation of the sea bottom between the Caspian and the Urals.

The same argument holds good as to the African mammalia, which could not have passed into Sicily, Spain, or Britain without a northward extension of the African mainland.

The relation of the Pleistocene to the Pliocene fauna is a question of great difficulty. If the Pliocene fauna be compared with that of the Forest-bed, it will be seen that the difference between them is very great. The Pliocene mastodon and tapir, and most of the Cervidæ, are replaced by forms such as the roe and red deer, unknown until then; but many of the Pliocene animals were able to hold their ground against the Pleistocene invaders, although they were ultimately beaten in the struggle for existence by the new comers. The fauna which the author adopted as typically Pliocene is that furnished by the lacustrine strata of Auvergne, the marine sands of Montpellier, and the older fluviatile strata of the Val d'Arno.

3. *On Glacier Motion;* by John Aitken.—In making some experiments on the freezing of water some time ago it was noticed that after the same water had been melted and frozen a number of

times it generally burst the tube in which it was frozen. On looking for an explanation of this phenomenon, it became at once evident that the experiment contained the germ of the explanation of glacier motion. Every time the water was frozen in the tube there was a mimic representation of glacier motion. The ice possessed, the first two or three times it was frozen, a certain amount of viscosity which enabled it to adapt itself to the shape of the tube, as was evident from the distortion of the upper surface of the ice in the tube. How came the ice to lose this plasticity or viscosity, this power of adapting itself to the shape of the tube, the loss of which caused it to burst the tube after it had been frozen and melted a number of times? Wherein did the ice which had only been frozen once differ from the other? The answer to this seemed to be, that the ice which had only been frozen once had more air in it than that which had been frozen and melted a number of times, as each succeeding freezing deprived the ice of a quantity of air or some other gases. The natural conclusion, therefore, seemed to be, that ice with air in it is a viscous substance, though pure ice is not. The first question then to be asked is, is ice with air in it a viscous substance? In order to get an answer to this question, glass tubes ·4 inch in diameter and twelve inches long were filled with water in which was dissolved a great quantity of air. The tubes were then placed in a freezing mixture. After the water was frozen in the tubes the tubes were slightly heated and the rods of ice withdrawn from them and placed on two supports eight and a half inches apart, and a weight of one pound hung from the center of these ice beams. The beams at once began bending and continued bending so long as the weight was left on them, thus proving the viscosity of the ice experimented on. The ice of these beams though similar was not the same as glacier ice; other ice beams were also made, in as close imitation of glacier ice as possible, which was done by placing a small quantity of water in the tubes, then some snow, and pressing it firmly to the bottom of the tubes, then adding more snow, and again firmly pressing it down, and so on till the tubes were filled, as much pressure being applied as possible to the snow to drive out the water. The tubes were then placed for some time in the freezing mixture. The ice beams were afterward withdrawn from the tubes and placed on the supports, and a weight of one pound hung from the center. The beams of snow-ice so made were found to be more easily bent than those made from the water. The rate at which they bent varied, possibly owing to there being more or less water-ice mixed with the snow-ice: one of the beams bent one inch in five minutes. Temperature seemed to have some influence on the rate of bending of these beams, but this point was difficult to determine on account of the different beams bending at different rates at the same temperature; but so far as could be ascertained from the experiments, the beams bent slower the lower the temperature. The lowest temperature used in these experiments was rather more than three Fahrenheit degrees below freezing.

Smaller rods of snow-ice were made ·2 inch in diameter, and as it was found that these could be easily bent in the hand, it was thought possible to bend them into rings. In attempting to bend these rods round a cylinder three inches in diameter, a difficulty was met with. After the pressure had been applied a short time, and before the circle was half turned, the rods always broke with a pressure which they easily bore at the beginning. Here, then, was a difficulty. The explanation seemed to fail at the last moment. The bending had so altered the structure of the ice, that it had lost much of its viscosity and became brittle. How then are we to account for glacier ice keeping its viscosity after years of bending? On examining the fracture of the beams it appeared as if a fibrous structure had been developed in the ice by the bending. The fracture did not go straight across, but part of it ran parallel with the axis of the beam, strongly resembling the fracture of poor bar iron, crystalline at one part, fibrous at another. The bending of the ice had evidently developed a laminated structure in it, similar to that found in glaciers. This laminated structure was developed along the beams, as was to be expected; for the direction in which this structure will be developed depends more on the direction in which the particles of ice are caused to slip over each other, than on the direction in which the pressure or tension is applied. The bending having produced this laminated structure in the ice, it is evident that the beams will be weaker after this structure is developed than before, on account of the cohesion of the ice being weakened along the planes of lamination. It was thought therefore that if the pressure was taken off the ice so as to relieve the particles from strain and stop them sliding over each other, that the laminæ which had been developed in the ice would, so to speak, become welded together, and the strength and plasticity of the beam be restored. Acting on this supposition, an attempt was again made to bend the ice beam into a circle. After a small part of the circle had been turned, the pressure was taken off the beam and a short time given for the particles to rearrange themselves; the pressure was then again applied, a small part more bent, and so on. When done in this way it was found that the ice beams were easily bent into a circle; the ends were then united by means of pressure, and a solid ring was thus produced from a straight beam of ice. These conditions of alternate rest and pressure are in all probability those which exist in glaciers. After pressure has acted at one part of the glacier, bending takes place, so relieving the ice at that part from the pressure, which comes to bear on another part of the glacier; and before the pressure again comes to bear on the first part its strength and plasticity or viscosity has been restored by rest.

Although ice under certain conditions has by these experiments been shown to be a viscous substance, to have the power of changing its shape and so enabling it to flow—though slowly—in its channel; although it has thus been shown that the viscosity of ice is a course of glacier motion, yet it must not, therefore, be concluded that it is the only cause. Among other causes which may assist in producing glacier motion may be mentioned: 1st. The sliding of the ice over its channel; this sliding being assisted by the tendency which the ice has to melt where it rests on its channel. 2d. The melting of the ice in front of obstacles, the melting being produced by the melting point of the ice in contact with the obstacle being lowered by the pressure of the ice behind. 3d. The melting of the ice in the body of the glacier, by heavy pressure being brought to bear at certain points, part of the water so formed finding its way to the channel under the ice, and part being re-frozen. 4th. The crevices in the glacier formed by the fracture of the ice. This breaking up of the ice will enable large masses of ice to move into different positions relative to each other, much more easily than if the ice was solid. This breaking up of the ice will also make the motion due to its viscosity take place quicker than if the ice was in one mass. 5th. The old dilatation theory explains something of the motion of glaciers, though it may not explain how that motion takes place; yet it accounts for some of the pressure which produces that motion.—*Nature*, Feb. 13.

4. *Note on Earthquake Waves;* by J. E. Hilgard, Assistant in Charge, U. S. Coast Survey. (Communicated to this Journal, with the sanction of Prof. Peirce, Director U. S. Coast Survey).—The self-registering tide-gauges, maintained by the U. S. Coast Survey at different points on the sea coast, frequently have exhibited, superimposed upon the tidal fluctuation, a succession of long waves, the origin of which is ascribed to distant earthquakes. In two notable instances, viz., the earthquake of Simoda in 1854, and that of Arica in 1868, the great ocean waves caused by the disturbance were distinctly registered in that way by the tide-gauges on the Pacific coast, and they have been made use of to estimate the average depth along the lines of transmission. See Coast Survey Reports for 1855, '62 and '69.

Similar fluctuations were registered on the morning of the 17th of November, 1872, shortly after local midnight, on the tide-gauge at North Haven, on the Fox Islands, in Penobscot bay, Maine. The fluctuations continued from midnight until nearly six o'clock in the morning, at somewhat regular intervals of about 17 minutes from crest to crest, with an average vertical range of 9 inches, the greatest wave being at three o'clock, with a height of 20 inches.

No corresponding earthquake phenomena have come to the knowledge of the Coast Survey office, and it is probable that if such was the case, the shock occurred somewhere under the Atlantic ocean.

5. *On the age of certain beds of Wyoming referred to the Tertiary by Prof. Hayden and to the Cretaceous by others;* by Prof. Lesquereux.—In a paper published as *rectification* by Prof. E. D. Cope, (Feb. 7, 1873,) and distributed as a circular, I read the following remark: "Prof. Lesquereux (Hayden's Survey of Territories, 1870, p. 306) had considered the fossil flora of Point of Rocks,

forty miles westward, as of "unknown age," and those of Evanston as "*Miocene.*"

The Report, loc. cit., p. 306, under: "3d column, *Eocene*," has:
"Mississippi flora from Hilgard's and Safford's specimens."
"Marshall mine."
"Raton pass, with Purgatory Cañon and Golden City."
"Washakie station."
"*Evanston*, below coal."
"Evanston, above coal, etc., etc."

Concerning Green river and Point of Rocks, I remark, p. 305: "The fourth section, marked *unknown* has the species from localities not satisfactorily known, either on account of the too small number of specimens representing them, or from want of reliable reference." The description of species of this volume shows that there were then only two species known from Green river: *Ceanothus cinnamomoides*, sp. nov., and *Carya Heerii* Ett., and from Point of Rocks also two species, a *Cyperites* and *Fagus Antipofii* Heer. From this I was not authorized to draw any conclusion. But in the Appendix to the same report, May, 1872, prepared from specimens received after the printing of the first part, twenty-two species are described from Green river, and the General Remarks, p. 17, concludes as follows: "The relation of all these species, except Cyperaceæ, etc.; found everywhere, is evidently with younger types and indicates a higher station in the Tertiary measures. From the absence of the species which characterize the American formation considered as Eocene, and also from the absence of Arctic types, which become less predominant in advancing toward our present epoch, the fossil plants of Green river apparently represent the Upper Eocene." Of Point of Rocks station, in same Appendix, p. 18, I described nine species in addition to the two formerly known from report, p. 289, and did not find in this group any species characteristic of a peculiar stage of the Tertiary, only remarking, "that three of these species are represented at Evanston, etc., in strata considered as Eocene, but that from the presence of Arctic types, which are not found at Green river, these strata occupy a lower stage in the Tertiary, though higher than Evanston, and that therefore its place is in the Lower Miocene." And same page 18 of the Appendix, the conclusion of the remarks on Evanston is as follows: "It is undeniable that without any exception, most of these types of ours, compared with European fossil species, should be referable to the Miocene. But, as said in the Report, pp. 313 and 314, either these species belong to the American Eocene, or as yet this formation is unknown in our geology."

These observations do not tend, in the least, to eliminate the priority of Prof. Cope to the discovery of the so-called Cretaceous characters of the group under consideration. These characters have to be critically reviewed in another place. They merely determine positively for future reference the conclusions indicated by botanical paleontology, even from the examination of a limited number of specimens.

Columbus, O., Feb. 15, 1873.

6. *Meek on the Cretaceous in Utah.*—In Dr. Hayden's report for 1870 (Preliminary Report of the U. S. Geol. Survey of Wyoming, &c.), Mr. Meek expresses himself as follows with regard to the Cretaceous age of certain beds at Coalville, Utah.

Some of the specimens from near Bear River, and at Coalville, Utah, from a light-colored sandstone, containing beds of a good qualtity of brown coal, appear to belong to a member of the Cretaceous series not corresponding to any of those named in the Upper Missouri country; though it is, as I believe, represented by a similar sandstone under the oldest estuary Tertiary beds at the mouth of the Judith River, on the Upper Missouri. In 1860 Colonel Simpson brought from this rock, on Sulphur Creek, a small tributary of Bear River, in Utah, some casts of *Inoceramus*, and other fossils; and in some remarks on Colonel Simpson's collection, published by the writer, in connection with Mr. Henry Engelmann, the geologist of Colonel Simpson's survey,* we referred this formation to the Cretaceous. The collections that have since been brought in from it, in Utah, by Mr. King's and Dr. Hayden's Surveys, confirm the conclusion that it belongs to the Cretaceous, as they contain, among other things, species of *Inoceramus*, *Anchura*, and *Gyrodes*—genera that seem not to have survived the close of the Cretaceous period. In addition to this, there is among Dr. Hayden's collections from this rock, at Coalville, a *Turritella* that I cannot distinguish by the figure and description, even specifically, from *T. Martinezensis*, described by Mr. Gabb, from one of the upper beds in California referred to the Cretaceous. A *Modiola* from the same horizon also appears to be specifically identical with *M. Pedernalis* of Roemer, from the Cretaceous of Texas. Dr. Hayden also has, from a little above the coal beds at Coalville, specimens of oyster that seem much like *O. Idriaensis* and *O. Brewerii* of Gabb, from the upper beds of the California Cretaceous. As no other fossils were found directly associated with these oysters, however, nor any strictly marine forms above them, it is possible that they may belong to the lower Tertiary.

From the affinities of some of these fossils to forms found in the latest of the beds referred in California to the Cretaceous, and the intimate relations of these marine coal-bearing strata of Utah to the oldest Tertiary of the same region, and the apparent occurrence of equivalent beds bearing the same relations to the oldest brackish-water Tertiary beds at the mouth of Judith River on the Upper Missouri, I am inclined to believe that these Coalville beds occupy a higher horizon in the Cretaceous than even the Fox Hills beds of the Upper Missouri Cretaceous series; or, in other words, that they belong to the closing or latest member of the Cretaceous.

7. *Supplementary Note on the Dinocerata;* by O. C. Marsh.—After the article on page 293 was printed, and copies distributed, another paper by Prof. Cope on the same subject was received (March 20th). In this paper, which is dated March 14th, 1873, and illustrated by four plates, Prof. Cope has at last adopted

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* See Proc. Acad. Nat. Sci., Philad., 1860.

nearly all my views as to the characters and affinities of the *Dinocerata*, as well as most of my corrections of his errors, although without giving credit in either case. Unfortunately, he still misinterprets the structure of this group on several points, and most of his dates are as incorrect as before. On nearly every page of the paper, moreover, new errors may be detected, a few only of which can be corrected here for want of space.

1st. Prof. Cope is wrong in assigning only three sacral vertebræ to the *Dinocerata*, as *Dinoceras*, the type of the group, certainly has four, and the other genera probably as many. 2d. The neck in *Tinoceras grandis* Marsh (or ? *Tinoceras cornutus*) was much more than a foot in length, rather than less, as the cervicals in the Yale Museum clearly prove. 3d. Prof. Cope is entirely in error in saying that the muzzle in this species could not reach the ground by several feet; the animal really having no use for the long proboscis which Prof. Cope insists in putting on him. 4th. The specimen described as *Eobasileus cornutus* was fully adult, as the teeth show, and the differences between it and the type of *Tinoceras grandis* may be due to age. 5th. The nasal bones in this genus do not form the inner half of the middle horn-cores, but only a small portion of the base, the cores being essentially on the maxillaries. 6th. The anterior extension of the malar bone is not in *Dinoceras* much less than in the perissodactyls. 7th. The tusks figured in plate I of Prof. Cope's paper are not in their true position, and in plate II the left tusk is placed on the right side, thus entirely reversing its characters. 8th. The name *Loxolophodon* was not applied to the genus *Tinoceras*, Aug. 19th, 1872, but long afterward, and then altered to *Lefalophodon*, with specific names all different from those now claimed. A good example of the inaccuracy which seems inseparable from Prof. Cope's work is seen in the explanation of the plates of this paper, where two serious mistakes occur in the first line.

Prof. Cope concludes with some remarks about nomenclature, evidently aiming to save, if possible, some of his names which are anticipated by mine. His views as to what constitutes publication are absurd, and would not be accepted by any scientific authority. His precepts about describing genera may be fitly compared with his practice, without going beyond the *Dinocerata*. The name *Loxolophodon* Cope was first given, without description, to a genus which Prof. Cope now rejects, and when again applied, contrary to usage, to the genus *Tinoceras*, all the generic characters mentioned existed only in that author's imagination.

Yale College, March 22d, 1873.

8. *Notice of Fossil Vertebrata from the Miocene of Virginia.*—Prof. Leidy directed attention to some fossils, part of a small collection recently received. They were found imbedded in blue clay containing an abundance of fossil diatomes, among which Coscinodiscus is especially conspicuous. The fossil vertebrate remains consist mainly of vertebræ and teeth of cetaceans, vertebræ of bony fishes, teeth of sharks, and spines of rays. Among them

also there is a portion of a humerus of a bird, and several worn teeth of a peccary. Besides these there are specimens which may be regarded as characteristic of the following undescribed species.

Protocamelus Virginiensis. Represented by the lower last premolar, and the first and last molars of an animal about the size of the existing Lama, and intermediate in size to *Protocamelus occidentalis* and *P. gracilis* of the tertiary of the Niobrara river, Nebraska.

Tautoga (*Protautoga*) *conidens.* Represented by a premaxillary with teeth, and portion of another with the first tooth. The specimens indicated a much larger species than the living Black Fish, *Tautoga.* The bones and relative position of the teeth exhibit some peculiarities. The premaxillary externally is flatter than in the Black Fish, and it appears as if it had not turned down in a hook-like end at its outer extremity. The teeth also are separated by comparatively wide intervals, independently of the interspaces provided for successional teeth. The form of the teeth is the same as in the Black Fish. One of the specimens contains the base of the first large tooth, and a row behind of seven other teeth. The other specimen contains the first large tooth, which is nearly half an inch in length, but proportionately more robust than in the Black Fish.

Acipenser ornatus. Founded on a dorso-lateral plate indicating an extinct species of sturgeon of medium size. The length or height of the plate is about 2½ inches; its breadth along the crest is an inch and three-fourths.—*Proc. Acad. N. S. Phil.*, 1872.

10. *Henry Woodward on Coal-measure Spiders.*—In the autumn of 1871, Mr. Woodward described, in the Geological Magazine, a very perfect specimen of spider from the Coal-measures of Dudley, which he named *Eophrynus Prestvicii.* In the same magazine, for Sept., 1872, he announced the discovery of another species, from Lancashire, occurring in the iron-stone and Coal-measures of that county. The specimen is 16 millimeters long and 7 broad. He referred it to the genus *Architarbus* of Scudder, and named it *Architarbus subovalis.* Scudder's species, *Architarbus rotundatus* was found in an iron-stone nodule at Mazon Creek, near Morris, Grundy Co., Illinois, and is described and figured in the 3d volume of Worthen's Illinois Geological Report (1868).

11. *A Monograph of the British Fossil Crustacea belonging to the order Merostomata;* by HENRY WOODWARD. (Printed for the British Paleontological Society). Part IV of this very valuable monograph was published during the past year. It contains descriptions of species of *Stylonurus*, *Eurypterus* and *Hemiaspis*, with wood-cuts and several lithographic plates, giving full-sized representations of these remarkable species. *Eurypterus ?* (*Arthropleura*) *ferox* of Salter, from the nodules in the Coal-measures of Tipton, Staffordshire, is probably, according to Woodward, a Myriapod of the genus *Euphoberia* of Meek and Worthen.

12. *Naumann on Pseudomorphism.*—In the December number, 1872, of the *Jahrbuch für Mineralogie und Palæontologie* of

Leonhard and Geinitz (which monthly Journal, by the way, contains the fullest record of new discoveries and observations in mineralogy and geology in any scientific periodical), Naumann has a letter correcting in strong language the statement of his views on pseudomorphism made by Prof. T. Sterry Hunt before the American Association in 1871. The letter closes with the remark that "only an incomprehensible misunderstanding can account for his statement, which has been already sufficiently refuted by Dana in the American Journal of Science for February and August of 1872."

13. *The Geological and Natural History Survey of Minnesota.* First Annual Report for 1872, by N. H. Winchell, State Geologist., St. Paul, Minn., 1873.—Prof. Winchell prefaces his valuable report by a list of the published books and articles that bear on the Geology and Natural History of the region, commencing with Father Hennepin's work published in 1679, and adding some notice of their contents. The topography of the State is described and the heights along various railroad lines are given. The distribution and relations of the several geological formations are briefly explained, and illustrated on a colored "preliminary geological map" of the State. This map shows that the Archæan (Azoic) rocks which border the west shore of Lake Superior extend southwestward through the middle of the State, and also along its northern border. East and west of the central Archæan axis there are the successive formations of the Lower and Upper Silurian, commencing with the Potsdam Sandstone; but those of the Upper Silurian have not yet been particularly studied. The Devonian is not known to be represented, although supposed to exist. In addition, the author describes the Cretaceous formation as covering most of the southern half of Minnesota, and probably a wide belt along its western border reaching to the United States boundary. The eastern limit of the Cretaceous is still uncertain. He supposes that it may formerly have overlaid a large part of the Archæan region. The report closes with a list of herbaceous plants in the vicinity of St. Anthony, drawn up by Prof. E. H. Twining.

14. *Large Diamond.*—A diamond weighing 288½ carats and of the first water, was found Nov. 6th, 1872, at Waldeck's placer, Vaal river, South Africa, by Robert Spaulding's party. It is stated to measure about 1⅛ inch in diameter. If this statement is confirmed the Waldeck-Spaulding diamond is among the largest rough diamonds of which we have mention. The Regent weighed 410 carats (136$\frac{14}{16}$ cut), and the Great Mogul 780½ carats (279$\frac{9}{16}$ cut). A diamond in the possession of the Rajah of Maltan in Borneo, weighs 367 carats; the Nizam belonging to the king of Golconda weighs 340 carats. The Mining and Scientific Press of Feb. 22, gives a figure of the Waldeck-Spaulding stone, taken from a photograph, which shows its form to be an irregular octahedron.

15. *Trautwinite.*—E. Goldsmith has thus named (Proc. Acad. N. Sci. Philad., 1873, p. 9), a green mineral occurring in micro-

scopic hexagonal crystals (pyramids with the prism, the latter sometimes 3-sided) on chromite from California, specimens of which he received from Mr. John C. Trautwine. Chemical and blowpipe examination showed that it contained oxides of chromium, iron and magnesium. Heated to redness in the closed glass tube, it gave a little water and turned bluish green. Not dissolved in acids.

16. *Mineralogische Notizen*, von FRIEDERICH HESSENBERG. No. 11. (From vol. viii, of the Abhandlungen of the Senckenberg Natural History Society of Frankfort.)—Hessenberg continues here his crystallographic researches. He describes crystals of perofskite from Pfitschthal, of calc spar from Iceland and Andreasberg, and others of sphene and axinite; indicating for sphene some evidence of hemimorphism.

17. *The Sexes of Sphæroma*; by OSCAR HARGER.—In a recent memoir on Crustacea of the Coast of France, published in the Annales des Sciences Naturelles, 5^e série, tome xvii, 1872–3, M. Hesse has, with considerable hesitation, advanced the opinion that *Spæhroma* is only the female of *Cymodocea*, and that *Dynamene* is the female of *Næsea*. The hesitation of this author rests upon the fact, that the evidence in his possession was unsatisfactory and negative in character; and he laments his ill success in raising the young of these animals, significantly remarking of the offspring of *Sphæroma*, which he raised as far as the third moult, "Lorsque ces Crustacés sont parvenus à ce degré de transformation, ils ont la forme de leur mère, c'est-à-dire celle des *Sphéromiens*." In the case of *Dynamene* and *Næsea* less hesitation is expressed, and the absence, so far as known, of the former genus from this coast, cannot be considered as conclusive against the proposition. Neither has *Cymodocea* yet been obtained so far as I know on our coast, but *Sphæroma quadridentata* Say is not uncommon on the coast of New England south of Cape Cod, and, in any considerable collection of these animals, both sexes may be easily found. The sexes are readily distinguished by the second pair of pleopoda which, in the males, are furnished with a slender style, articulated to the inner or posterior lamella, and lying along its inner side. Near the middle of the ventral surface of the seventh thoracic segment of the males are two short, movable processes. In the females the plates, which during the breeding season are developed for the purpose of carrying eggs, are at other times small, and may be difficult of observation, but can usually be found as slender processes from the basal joints of the legs. Except in these sexual characters the two sexes closely resemble each other, and both belong to the genus *Sphæroma*.

18. *Key to North American Birds, containing a concise account of every species of Living and Fossil Bird, at present known from the Continent north of the Mexican and United States Boundary*; by Dr. ELLIOTT COUES, U. S. A. Large 8vo, 361 pages, with 6 steel plates and over 250 wood-cuts. Naturalist's Agency, Salem, Mass.—This is an excellent manual of North American ornithology, suitable for beginners in the science, and useful and con-

venient for those who are already proficient. It is beautifully printed on heavy paper, and the illustrations are appropriate and well executed. The typography of the work does great credit to the "Salem Press," where it was printed.

In the first part of the book the author describes and illustrates the plumage of birds, and gives an account of their external characters and anatomy, together with such explanations as seem necessary in respect to the principles of classification, etc.

This is followed by an artificial key to all the genera, which will be especially useful to those who are beginning the study by themselves.

The greater part of the volume is taken up by the systematic synopsis of the birds. In this part the author has given clear and concise descriptions of all the species, and generally of both sexes and the young, with their geographical range, and references to the principal works where they are described and figured. The descriptions of the genera and higher groups are also good. The syonymy has been mostly omitted. The synopsis of fossil species contains brief notices of the species hitherto described, with references to the original descriptions. This part has already been noticed in this Journal. A copious combined index and glossary closes the volume, which we heartily welcome as a very important addition to our educational works in Natural History. A. E. V.

19. *Arrangement of the Families of Fishes, or Classes Pisces, Marsipobranchii, and Leptocardii;* by THEODORE GILL. 8vo, 95 pages, Smithsonian Miscellaneous Collections.—This work consists, 1st, of an introduction in which the classification of fishes is discussed at considerable length, and the views of various other writers are presented, together with a discussion of the homologies of the Shoulder Girdle and Pectoral Limb; 2d, a list of the families and higher groups; 3d, a very useful Bibliography of the most important general descriptive works on Ichthyology; 4th, an alphabetical index to the names of families and other groups. As indicated by the title, Professor Gill recognizes among the so-called fishes three distinct classes of vertebrates. These classes do not, however, correspond to those recognized by Professor Agassiz. The former unites the Selachians, Ganoids and Teleosts as sub-classes of the class Pisces; while the latter has proposed to consider them as distinct classes. The following are the higher divisions adopted by Professor Gill:

Class Pisces.
    Series I.—Teleostomi or Branchiata.
        Sub-class 1.—Teleostei, including 9 orders.
        Sub-class 2.—Ganoidei, including 6 orders.
    Series II.—Elasmobranchii.
        Sub-class 3.—Elasmobranchii, including 3 orders.
Class Marsipobranchii.
        Orders Hyperoartii and Hyperotreti.
Class Leptocardii.
        Order Cirrostomi.

20. *Arrangement of the Families of Mammals, with Analytical Tables;* by THEODORE GILL. 8vo, 98 pages, Smithsonian Miscellaneous Collections, November, 1872.—This work is still incomplete. The part published contains, 1st, a list of the families and higher groups of mammals, with some of their synonyms; 2d, Bibliography of the works referred to; 3d, Synoptical Tables of Characters of the subdivisions of Mammals, with a catalogue of the Genera.

The Synoptical Tables are completed only to the end of the Cete. This part of the work gives a very convenient epitome of the principal characters of the groups, and it is to be hoped that it will soon be completed.

The classification will be indicated by the following table of the higher groups:

Sub-class I.—Placentalia or Monodelphia.
  Super-order 1.—Educabilia (=Megasthenes Dana).
    Eight orders: Primates; Feræ; Ungulata; Toxodontia; Hyracoidea; Proboscidia; Sirenia; Cete.
  Super-order 2.—Ineducabilia (=Microsthenes Dana).
    Four orders: Chiroptera; Insectivora; Glires; Bruta.
Sub-class II.—Didelphia.
    Order Marsupialia, with four sub-orders.
Sub-class III.—Ornithodelphia.
    Order Monotremata, with two sub-orders. A. E. V.

21. *Fertilization in Grasses.*—Professor HILDEBRAND of Freiburg made to the Berlin Academy a detailed communication on this subject, which is published in its Monatsbericht, Oct., 1872. He shows that there is an entire series of steps from the completely diœcious arrangement to that in which self-fertilization is the rule even if it has exceptions. There are, for instance, some examples of diœcious grasses, then a number of monœcious, after which follow some with both hermaphrodite and staminate flowers, where the latter only can serve for crossing; then, in greater number, grasses with purely hermaphrodite flowers; in some of which the pistil develops before the anthers; in others, where the pistil and anthers develop simultaneously, the discharge of pollen from the anthers lasts for an appreciably longer time; but there are some cases where the pistil and anthers appear to develop together and have the same duration, but yet under such conditions that the pollen can reach the pistil only with difficulty. And finally some grasses in which close fertilization is not avoided, but actually occurs in a large proportion of cases, and even preponderates; yet even in these instances occasional cross-fertilization does not appear to be excluded

So that fertilization in grasses, as in other families of plants, must be studied, species by species, and we cannot apply our observations of one species to another species even of the same genus. Thus the genera *Hordeum*, *Avena* and *Triticum* exhibit great diversities in respect to fertilization in their several species.

A. G.

III. ASTRONOMY.

1. ***Meteors** of November 27th, 1872, and **Biela's** Comet.*—om various parts of Europe the accounts of the meteors con-ue to come to us. Schiaparelli and Padre Denza have given to e *R. Inst. Lombardo* a summary of the observations received them (*Rendiconti*, vol. 5, Fasc. 20), and Prof. Tacchini has blished those made in Sicily. In the *Wochenschrift* Prof. Heis s published a large number of accounts, especially those of servers in Germany.

One interesting question, whether or not the comet observed Pogson at Madras on the 2d and 3d of December can be one of e fragments of Biela's comet, is discussed by Klinkerfus (*Astron. tices*, Jan., 1873), Peters, Holetschek and Oppolzer (*Astron. ch.*, 1917 and 1920). Perhaps the most satisfactory treat-nt of the question is by Oppolzer. It will be recollected that gson made two observations, at about 24 hours intervals, and at the comet during that time moved 1° 40′ in declination, and ^{m} 36^{s} in right ascension. More places are needed to give the bit of the comet. Can these two be represented by an orbit e that of Biela? On the one hand, Klinkerfus asked Pogson look near *Theta Centauri* for the comet; he looked, and saw it. the other hand, the direction of the motion is rapid, and is ither from nor to the point opposite to the radiant in Andro-da. Again, this comet was twelve weeks behind the computed ces of Biela. Prof. Oppolzer, at the request of Dr. Weiss, amined the observations, beginning his work with the impres-n that the Pogson comet was not one of Biela's, but he became nvinced by the result of his computations that the likeness of e elements which he obtained to those of Biela could not be the ult of chance.

It seems to us extremely improbable that the comets have lost elve weeks, as there have been apparently no large doubtful rturbations. Through the last 20 years Jupiter has kept at a nsiderable distance from them, and his influence has been owed for. The natural deduction is, as stated by Mr. Proctor, at what Mr. Pogson saw was a meteoric aggregation traveling the track of the comet, but far behind it. In fact, it seems to a third piece of Biela's original comet, separated from the in body, perhaps, long ago. The size of the nucleus seen by r. Pogson was comparable to that of the moon, allowing for the obable distance of the comet. It took us over six hours to cross e dense part of the meteor stream on the 27th of November. d its thickness been only equal to that of the moon's diameter, e shower would have lasted about 10 minutes.

The meteors seen on the evening of Nov. 24th appear to have longed to still another stream of fragments from Biela's comet, ce they were not to be seen in so great numbers on the evening the 25th. Possibly the Pogson comet belonged to that stream.

Capt. Tupman suggests that the two observations of Mr. Pogson were of the two different known parts of the comet.

The brilliant shower, and the subsequent appearance of the comets, cannot fail to recall the closely analogous case of the shower of 1366, and of the two comets seen immediately thereafter, traveling apparently in the path of the meteoric stream.* In that case, however, the comets were seen after the earth passed the node, but before the comets reached it. H. A. N.

2. *Meteor in Kentucky, Dec. 12th,* 1872.—Under date of Dec. 12th, Prof. Kirkwood writes: "At 4^h 53^m this evening (just after sun-set), I saw a magnificent meteor. It was observed through a southern window, and came into view about 15° east of the meridian, at an altitude of 40° E. Its course was east of south, making an angle of 60° with the horizon. Its light was intensely brilliant. It exploded at an altitude of 10°, but I heard no report."

Prof. Kirkwood has also sent several notices from the papers. The *Lebanon Standard* (Marion Co., Ky.), says that a bright white light was seen in a N.W. or N.N.W. direction at an elevation of about 45°, at first inclined to the horizon but almost instantly assuming a position perpendicular to it. The smoke remained several minutes, separating into parts and fading away. An explosion was heard in Georgetown, Ky.

A letter from Prof. White, of Lexington University, states that it was seen (by a person five miles N.W. of that city) to pass from a point probably about 30° west of the zenith, to a point nearly southwest of the observer. A heavy explosion was heard five minutes after the disappearance of the meteor. The cloud remained several minutes.

The various accounts are conflicting, but are best explained by a meteor moving about S. 45° E., and exploding at an altitude of about 20 miles. The inclination to the horizon is quite uncertain, probably not less than 30°, and not more than 60°. If fragments came to the earth, it was probably in a district northwest of Lebanon, and not more than twenty or thirty miles from that place. H. A. N.

3. *Double Meteor of Feb. 14th,* 1873.—A meteor, or rather a small group of meteors, appeared near the planet Venus, as seen at New Haven, just after the clock struck six on the evening of Feb. 14th. They moved northward and downward to an altitude of 15°, in the direction N. 64° W. Mr. William C. Wood, who was with the writer, saw them during about ten degrees of their track; I saw them only for an instant. There were two balls, the leading smaller one being bright green, and the following one of a yellowish color. Other observers saw three balls. From the incipal ball Mr. Wood saw sparks separate. It was less bril- [illegible] than Venus. The same meteor was seen by Rev. Mr. Middle- [illegible] New Britain, who saw it divide into two large portions, ne or two smaller ones. The small soon vanished, while the rge ones passed on.

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* This Journal, II, xlv, 91.

This meteor must have been seen at many places west of New Haven, and accounts of its appearance elsewhere are respectfully solicited. H. A. N.

4. *Astronomical Engravings from the Observatory of Harvard College.*—Eighteen plates have been issued out of the thirty promised (vol. iv, p. 243), from the Observatory of Harvard College. The first two plates contain eight views of Jupiter, taken last spring. They exhibit minutely the structure of the belts, and especially the red equatorial belt, a feature very conspicuous then, but now, as Prof. Winlock informs us, no longer visible. There are four plates containing twelve views of the whole solar disk. Among them those which are copied from photographs made by a stationary telescope of long focal distance should be especially noticed. Plate 5 gives six views, on successive days, of a notable sunspot with its bridges, or tongues of light, its faculæ, and its other features, and plate 14 gives other sunspots. Three plates are devoted to various forms of solar prominences. There are three plates giving eleven views of lunar mountains. A fine view of Saturn, two copies of Eclipse photographs, and one plate giving views of the Spectroscopes used, make up the list of plates thus far published.

We most cordially commend these engravings. A teacher of a class in Astronomy could not find in any other form, for the same price (ten dollars), that which will help him so much in his work, as a set of them. If he has a telescope, the engravings suggest the points to be looked at, and if not, they are the very best substitutes we know of for the direct views of the heavenly bodies. They are, we understand, to be accompanied by explanatory notes. H. A. N.

5. *A new method of viewing the Chromosphere.*—Mr. Lockyer and Mr. Seabrooke have sent to the Royal Society a description of a proposed instrumental arrangement for viewing and photographing the chromosphere. The light of the sun is cut off by a circular disk, and the annulus surrounding the disk is viewed by the spectroscope. Drawings made by the new method were exhibited to the Society.

6. *The Chromosphere visible in small telescopes.*—It is a common idea that the hydrogen jets and clouds upon the border of the sun can be seen only through large instruments. Capt. Tupman has communicated to the Royal Astronomical Society a series of observations made by him with a three-inch telescope of indifferent character, and a direct vision five-prism spectroscope. The cost of the entire combination, including stand, was only 18 pounds.

7. *Papers relating to the transit of Venus in* 1874; Part II. 4vo, pp. 48, and 4 plates.—Mr. G. W. Hill, of the Nautical Almanac office, prepared, under the direction of Prof. Coffin, charts and tables for facilitating the prediction of the several phases of the transit of Venus. They were computed from Mr. Hill's Tables of Venus, and were intended as a supplement to the

Nautical Almanac, but were transferred to the Transit Commission, who published them. The four large charts accompanying the paper are suited to the use of navigators who may wish to observe the transit. They are, we may observe, remarkably fine specimens of map printing, the work, if we are not mistaken, of Mr. Bien.

8. *On the Auroral Spectrum.*—A letter from Henry A. Rowland, at present Instructor in Physics in the Rensselaer Polytechnic Institute at Troy, informs us that he observed the line of wave-length 431 in the auroral spectrum of last October. He says: "The observations were made with an ordinary chemical spectroscope of one prism, in which the scale was read by means of a lamp. Great care was taken in the readings, and after completing them the spectroscope was set aside until morning, when the readings were taken on the lines of comparison without altering the instrument in any way or even regulating the slit. The wave-lengths of the known lines were taken from Watts's 'Index of Spectra,' but as he does not give the wave-lengths of lines in the flame spectrum I am not quite certain that they are correct." On the scale of his instrument, Li α was at 13·5°, Ca α 21°, Na α 27·5, Ca β 36°, Ca γ 95·5°, and K β 110°. The aurora lines were as follows:

	Scale-reading.	Wave-lengths.
1	19	628·3
2	35·5	554·3
3	95	425

"The wave-lengths of the auroral lines were obtained by graphical interpolation on such a large scale as to introduce little or no error." G. F. B.

9. *Reports on Encke's Comet, and other reports from the U. S. Naval Observatory.*—From Admiral Sands we have received at different times the 2d, 3d, and 4th Appendices to the volume of observations for 1870, and the 4th Appendix for the volume for 1871, of the U. S. N. Observatory.

The 2d Appendix consists of reports by Professors Hall and Harkness on Observations of Encke's Comet during its return in 1871. Prof. Hall gives the positions of the comet, notes of its appearance, and four drawings of it. Prof. Harkness gives his observations upon the spectrum of the comet, followed by a discussion of the probable mass of the comet, and the density of the supposed resisting medium of space. The following is the general summary of his results:

(1.) Encke's comet gives a carbon-spectrum.

(2.) From November 18th to December 2d the wave-length of the brightest part of the second band of the comet's spectrum was continually increasing.

(3.) No polarization was detected in the light of the comet.

(4.) The mass of Encke's comet is certainly not less than that of an asteroid.

(5.) The density of the supposed resisting medium in space, as computed from the observed retardation of Encke's comet, is such that it would support a column of mercury somewhere between $\frac{220}{10^{17}}$ and $\frac{285}{10^{20}}$ of an inch high.

(6.) There is some probability that the electric currents which give rise to auroras are propagated in a medium which pervades all space, and that the spectrum of the aurora is, in reality, the spectrum in that medium.

(7.) It it not improbable that the tails of all *large* comets will be found to give spectra similar to that of the aurora, although additional lines may be present.

The *Third Appendix* is by Prof. Newcomb, on the right ascensions of the equatorial fundamental stars. His object is to do for the right ascensions of the equatorial and zodiacal stars, on which the reductions of lunar and planetary observations depend, what has been done by Dr. Auwers for the declinations, namely, to furnish the data necessary to reduce the principal original catalogues of stars to a homogeneous system by freeing them of their systematic differences. He gives a table of the right ascensions of the twenty-seven stars considered for each fifth year from 1750 to 1900, and the corrections for reducing the positions given in the several catalogues to the mean system.

The *Fourth Appendix for* 1870 contains reductions of zones of stars observed with the transit instrument in 1846, '47, '48, and '49. The corresponding observations of the zones with the mural circle were published as Appendix II. of the volume for 1869. The reductions in both cases were made principally by Dr. Gould, and the numbers of stars included in the two series are 12,033 and 14,804 severally, most of them being between 20° and 45° south declination.

The *fourth* appendix for 1871 is a memoir on the founding and progress of the U. S. N. Observatory, by Prof. J. E. Nourse.

The course of Admiral Sands in encouraging the professors in the observatory to bring out in their own name special researches, deserves a creditable notice. H. A. N.

10. *Note Spettroscopische sul Sole e gli altri corpi celesti;* by P. A. SECCHI.—This is a reprint of the original Spectroscopic Notices published by the author at various times, and in various journals, since the discoveries of Janssen and Lockyer in 1868.

11. *The Astronomische Nachrichten* will hereafter be sent from Kiel instead of Altona. Communications and subscriptions should be addressed to "*Professor C. A. F. Peters, Kiel, Sternwarte.*" (The subscription price is 3⅔ dollars, gold.)

IV. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *The California Academy of Sciences* has received from Mr. James Lick, a magnificent gift of a building site in the city of San Francisco, valued at about $100,000. By the terms of the gift it is required that the building to be hereafter erected upon it shall cover a space of 80 by 225 feet, be of three stories in height, built of brick with granite front, and ornamented with

scientific emblems; and the funds for this building must be secured within two years from the date of the deed. It is necessary to obtain about $100,000 in money to secure this gift, and the Academy have accepted the land in the conviction that they can raise the money. As remarked by the Mining and Scientific Press, Mr. Lick deserves the hearty thanks of the workers in the cause of science all over the world, since he has opened his purse spontaneously to give aid to the cause of science.

2. *The Owens College Junior Course of Practical Chemistry;* by FRANCIS JONES, Chemical Master in the Grammar School, Manchester. With a Preface by Professor Roscoe, F.R.S. 18mo, pp. viii, 171. London and New York, 1872. (Macmillan & Co.)—This little book is not offered as an exhaustive analytical manual, but aims to give only so much instruction as is needed by the average undergraduate student to make the reasoning in his course of chemical study intelligible. Hence the plan of the book is simple. Part I. is devoted to the preparation of and to experiments upon the different gases and some of their compounds. Part II. is devoted to blowpipe analysis; the section on Bunsen's flame-reaction, being most excellent. Part III. is occupied with the reaction of the metals, and Part IV. with those of the inorganic and organic acids and of the alkaloids. Part V. treats of the reactions of the rare metals, and an Appendix contains a table of the elements and a series of questions and exercises upon the entire book. With the able assistance of Professor Roscoe and of Dr. Schorlemmer, Mr. Jones has given us a valuable book, and one which has a most useful place to fill in instruction. We commend it to every teacher who can offer to his class any facilities for the practical study of chemistry. G. F. B.

3. *The Ancient Stone Implements, Weapons, and Ornaments of Great Britain;* by JOHN EVANS, F.R.S., F.S.A., etc., pp. 640, 8vo, with 476 wood-cut illustrations. New York, 1872. (D. Appleton and Company).—This extended monograph gives evidence of most careful study. It is a model for similar labors in other countries, and all who follow Mr. Evans in this line of work will be spared much preliminary labor in comparison and research, as well as in the classification and systematizing of materials so varied and yet so great. His book will be read with pleasure for its instruction, and referred to as an authority. The arrangement of the text is clear and methodical and copious indexes, general, geographical and topographical, with an analytical table of contents. render it of easy reference. The wood-cut illustrations show well the peculiarities of the implements represented, and are nearly all original. We know of nothing of the kind better done.

4. *Evolution of Life;* by HENRY C. CHAPMAN, M.D., member of the Academy of Natural Sciences, Philadelphia. 194 pp. 8vo. Philadelphia, 1873. (J. B. Lippincott & Co.)—This work is devoted mainly to illustrating the fact of the evolution of the systems of life, and to a display of the supposed succession of groups in the progressing evolution. The determination of the order of succes-

sion in the progress of life on the globe is one of the great objects of geological investigation, and is daily being furthered by new accessions of facts; but we think it too soon to make out genealogical trees with the detail attempted by the authorities followed in this work. He has high authority for tracing the vertebrates down to the Ascidians; but we believe, with Prof. Verrill, the conclusion to be based on a wrong idea of the structure of the latter class of animals. There is much that is interesting in the work; yet there appears to us to be too many "non sequiturs" to satisfy strict science. The volume has, as its frontispiece, a map entitled a "Hypothetical Sketch of the Monophylitic Origin, and of the Diffusion of the 12 varieties of Men from Lemuria over the Earth" ("Lemuria" being the supposed birth-continent of man, so named by Sclater from the Lemurs of Madagascar and the Indian Archipelago, and by him located in the ocean between Southern Asia and Eastern Africa); it appears to us to be *very* hypothetical. The work is illustrated also by a number of plates representing the structure and relations of various species of plants and animals.

5. *Address before the Royal Society of New South Wales, at the Anniversary meeting in* 1872, by the Rev. W. B. CLARKE, F.G.S., Vice-President.—The Rev. Mr. Clarke, during the past thirty five years a resident of New South Wales, has labored assiduously and successfully in the geological exploration of the country, and has perhaps done more than any one else in bringing to light its mineral resources. Even as early as 1841 he proved the existence of gold by discovering it in the granite rocks of the interior of New South Wales. The address, in connection with its Appendix, reviews many of the facts connected with the diamond and gold fields and tin and copper mines of Eastern Australia and some other British Colonies, and the results of recent explorations in Queensland.

6. PROF. TYNDALL'S *works.*—The recent visit of Prof. Tyndall to the United States has naturally excited a fresh interest in his works. Messrs. Appleton have published the following works of Prof. Tyndall.

I. Heat as a mode of motion. 1 vol., 12mo.

II. On Sound. A course of eight lectures delivered at the Royal Institution of Great Britian. 1 vol., 12mo.

III. Fragments of Science for Unscientific People. A series of detached essays, lectures and reviews. 1 vol., 12mo.

IV. Light and Electricity. Notes on two courses of lectures before the Royal Institution of Great Britain. 1 vol., 12mo.

V. Hours of Exercise in the Alps. 1 vol., 12mo.

VI. On Radiation. The "Rede" Lectures, delivered in the Senate House before the University of Cambridge, England, etc. 1 vol., 12mo.

VII. Forms of Water, in Clouds, Rain, Rivers, Ice and Glaciers, with a portrait of the author. 1 vol., 12mo.

VIII. Contributions to Molecular Physics in the domain of Radiant Heat. 1 vol., 8vo.

The same publishers have just now issued Prof. Tyndall's lectures in this country, in a duodecimo volume with numerous illustrations.

OBITUARY.

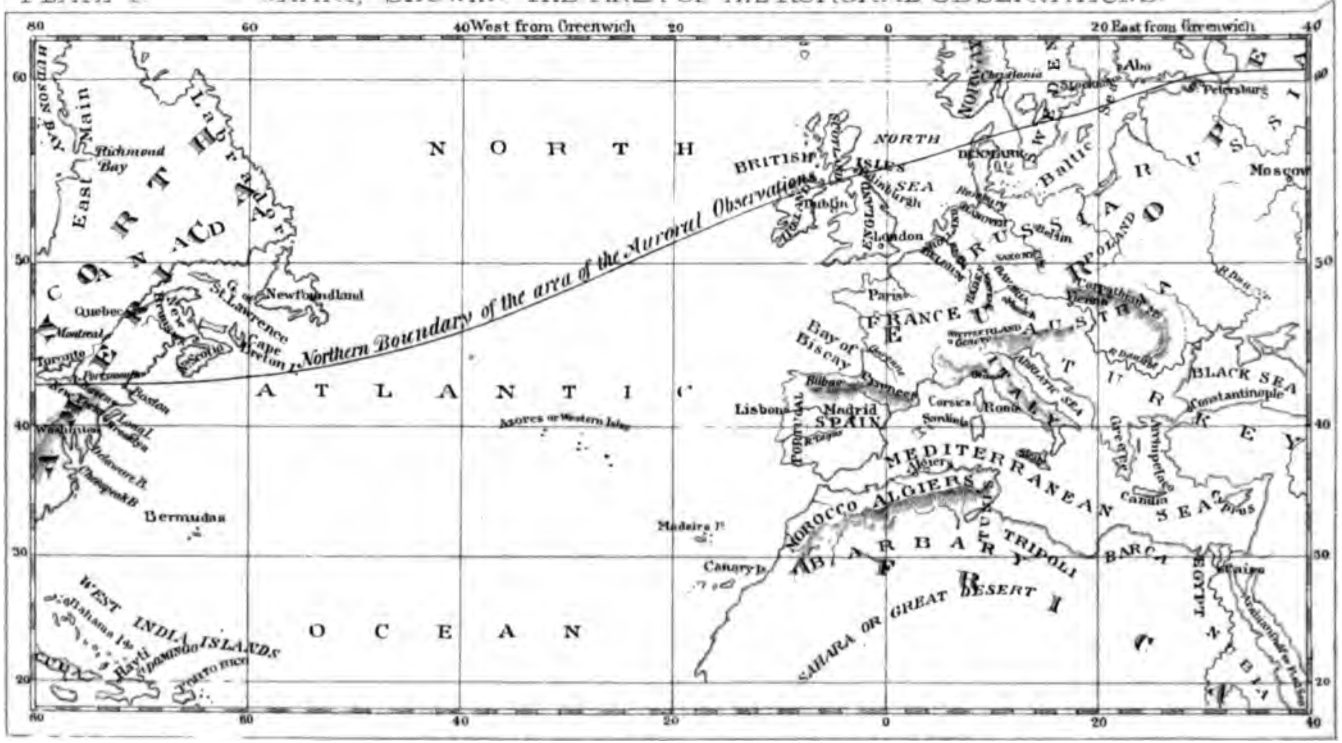
DR. JOHN TORREY.—Dr. Torrey died of pneumonia at his house, Columbia College, New York, on Monday, March 10th, at the ripe age of 77 years. He was born in New York, August 15th, 1796. He was universally beloved by all who knew him for his genial and truthful qualities, and equally respected for his high intellectual and moral powers and his solid attainments in various sciences. Immediately after his graduation in medicine he entered upon the study of mineralogy, chemistry and botany, the three sciences to which he devoted his life. His first botanical memoir, a list of the plants growing within thirty miles of the city of New York, was published, and his mineralogical contributions commenced, a little earlier. He was one of the founders and presidents of the New York Lyceum of Natural History, in the Annals of which some of his earliest papers appeared. He was made Professor of chemistry in the United States Military Academy at West Point in 1824, where he remained only three years, preferring the same chair in his Alma Mater, the College of Physicians and Surgeons in New York, where he entered upon his duties in 1827 and remained a most successful and honored teacher of his favorite science until 1854. During a portion of this time he also discharged the duties of the chemical chair in Princeton College, New Jersey. In 1853 Dr. Torrey became chief assayer of the U. S. Assay Office in New York, having refused the more responsible and remunerative position of Director from a characteristic unwillingness to enter upon duties which, in his judgment, required business qualities he did not possess. In this office he remained until the end, having signed his last report on the morning of Monday, only a few hours before his death.

In chemistry Dr. Torrey labored much in the laboratory, but he published little, his extreme modesty and conscientious desire for perfection seeming constantly to restrain his pen. His memoir on pectic acid in Tuckahoe or Indian-bread (*Sclerotium giganteum* Torr.) in 1827, and his early researches on the liquefaction of gases are evidence of his accuracy and zeal. He was well known to all students in mineralogy and chemistry for his acute perceptions and minute knowledge of the existing state of these sciences, in which he kept himself always well read up.

But the most important and numerous contributions of Dr. Torrey to science were made in the the department of Botany, of which he was an assiduous and most successful student up to the close of his life. Dr. Torrey's labors in this science will be chronicled in these pages by his fellow worker in the same field, Dr. Asa Gray, whose name is intimately associated with his in the history of American botanical science.

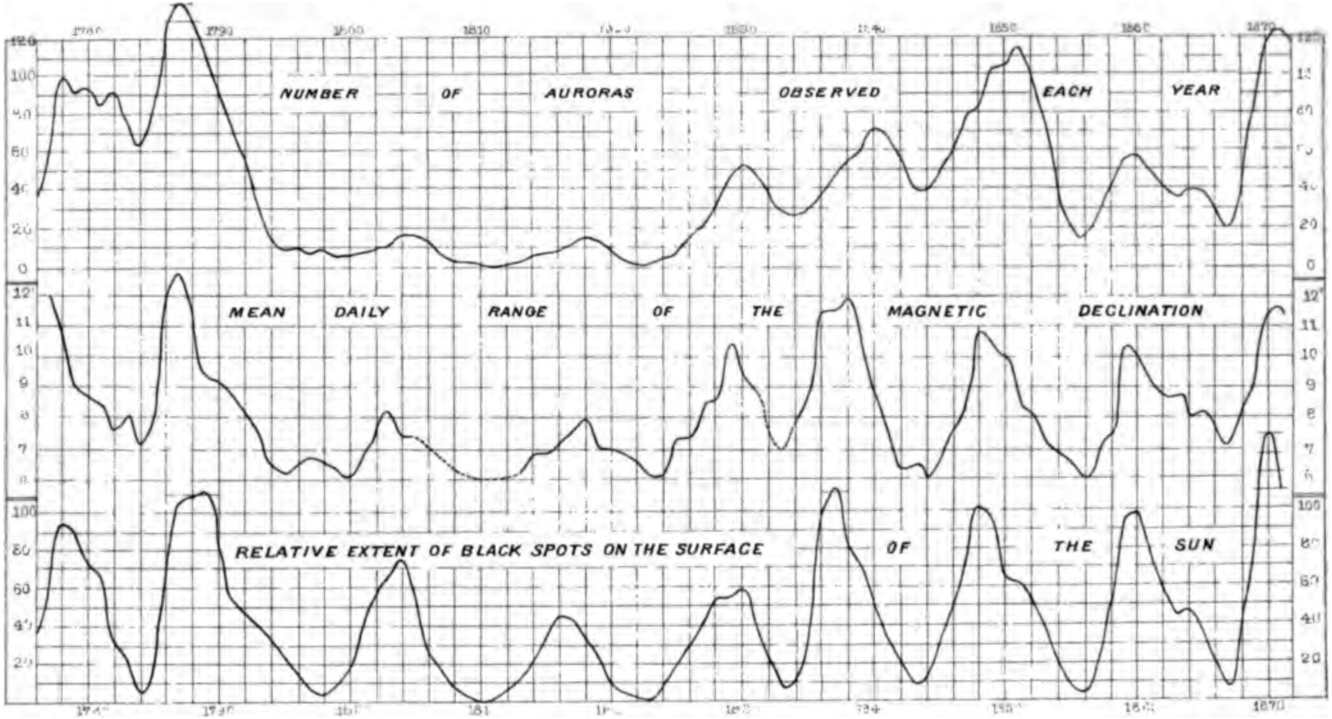
Dr. Torrey leaves one son, who was his official associate, and three daughters, all noted for their varied learning. B. S.

PLATE I. CHART, SHOWING THE AREA OF THE AURORAL OBSERVATIONS.

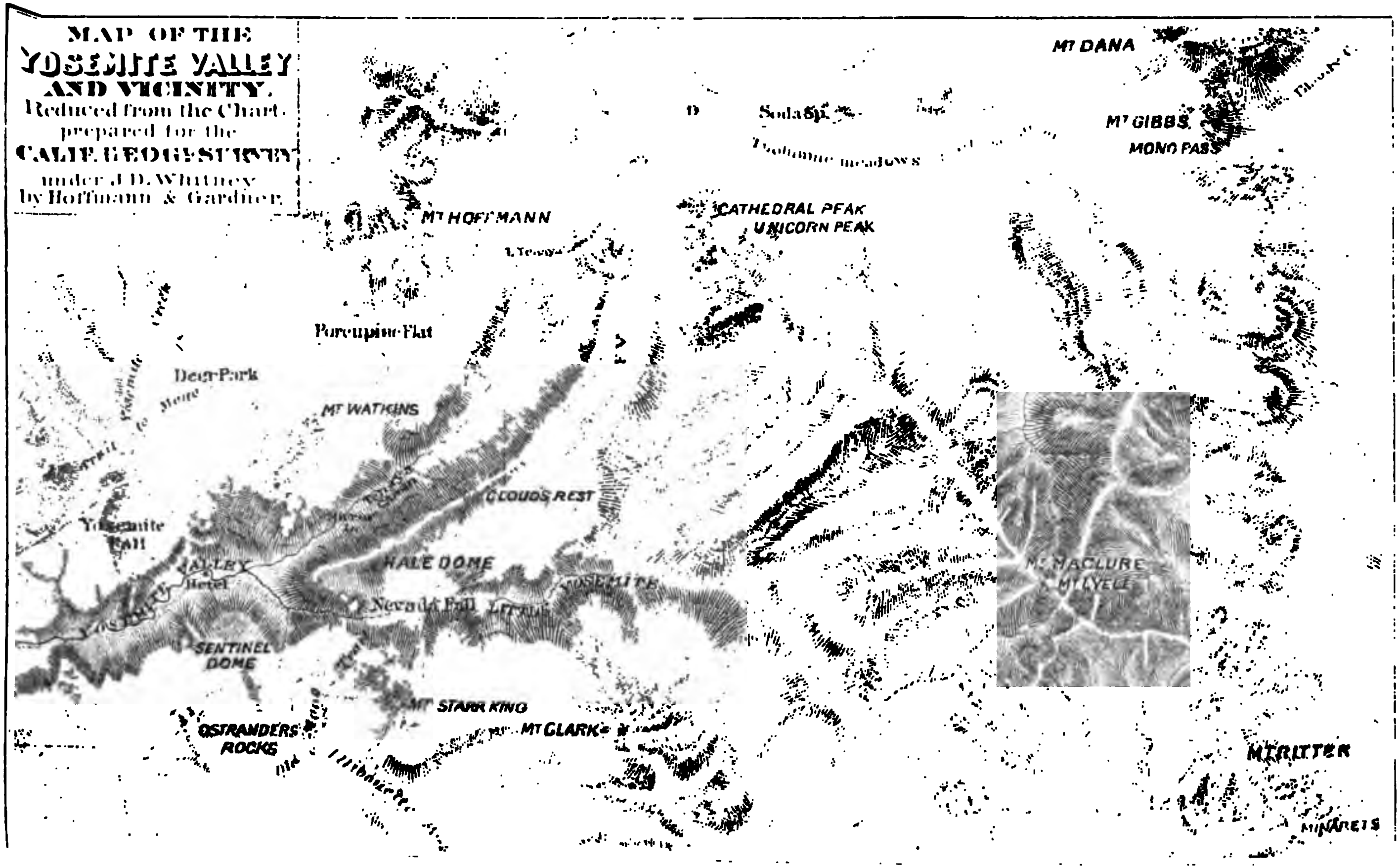


Punderson & Crisand, New Haven, Ct.

PLATE II. SOLAR SPOTS, MAGNETIC DECLINATION AND AURORAL DISPLAYS.



Punderson & Crisand, New Haven Ct.



MAP OF THE
YOSEMITE VALLEY
AND VICINITY.
Reduced from the Chart
prepared for the
CALIF. GEOL. SURVEY
under J. D. Whitney
by Hoffmann & Gardner.

THE

AMERICAN

JOURNAL OF SCIENCE AND ARTS.

[THIRD SERIES.]

Art. XXXVI.—*On some of the ancient Glaciers of the Sierras;* by Joseph LeConte, Prof. Geol. and Nat. Hist. Univ. of California.* With a Map on Plate V.

During the past summer, in company with several young men, mostly students and graduates of the University of California, I spent some four or five weeks in camp, among the high Sierras, examining the traces of the ancient glaciers of that region. Two years ago I carried a similar party over nearly the same ground. In addition to what has already been made known by the Geological Survey, I observed many phenomena which I think cannot fail to interest geologists and physicists.

That I may be clearly understood, I will very briefly describe my route.† From Oakland I went immediately to Yosemite, and spent about a week in that valley examining the evidences of glacial occupation. In this examination I was greatly assisted, in fact my observations were almost entirely directed, by Mr. John Muir, a gentleman of rare power of scientific observation, who has lived several years in the valley and devoted much time to glacial studies, and who undoubtedly knows more about the ancient glaciers in the vicinity of Yosemite than any man living. After satisfying myself that this magnificent valley, if not *formed*, was certainly *once occupied*, by a great glacier, I went up the Nevada cañon to the top of Nevada falls, and thence by the Mono trail (dotted line on map) along the margin of Little Yosemite Cañon, then up Feldspar valley (F. V.), by the base of Cathedral Peak, to the Tuolumne Meadows at Soda Springs. Here we left the Mono trail and went up the Tuolumne meadows to the foot of Mt. Lyell, and climbed this mountain. From Mt. Lyell we returned to the Mono trail, and followed it over Mono pass, down Bloody Cañon to Lake Mono and the extinct volcanoes in that vicinity. From Lake Mono we went northward, among the mountain valleys on the east side of the crest, to Lake Tahoe; and then by Johnson's pass and the Placerville road to Placerville and Sacramento and so back to Oakland. From the Yosemite to Mt. Lyell I was accompanied by Mr. Muir and Mr. Galen Clark, both men of rare intelligence and accomplished mountaineers.

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* Read before the Cal. Acad. of Science, Sept. 16, 1872.

† The map accompanying this paper (Plate V) is reduced from a part of the excellent chart by Hoffmann and Gardner, issued by J. D. Whitney in connection with the publication of the California Geological Survey under his charge.

During my previous visit the route was a little different. From Yosemite to Soda Springs I took the Coulterville Mono trail, which passes by Mt. Hoffman and Lake Tenaya. On that occasion too I ascended Mt. Dana. The whole distance (near 600 miles) in both cases was accomplished on horseback. I had, therefore, good opportunity and leisure for observing. I found abundant evidences of ancient glaciers in almost every part of the Sierra. Among those observed I will select a few of the most interesting.

I. THE YOSEMITE OR MERCED GLACIER.

There can be no doubt, I think, that the Yosemite valley was once filled to its *brim* by a great glacier. Whitney, in his Geological Survey, distinctly states his belief that a glacier once occupied this valley to the depth of 1000 feet; this belief being mainly founded upon the supposed remains of old moraines discovered by Mr. King.* But this opinion he subsequently retracted in his *Yosemite Guide Book*,† and now thinks there is no sufficient evidence of the former existence of such a glacier. In my opinion his first conclusion is undoubtedly the correct one.

It is true that *glaciated surfaces* are not made out with certainty in the main valley, and the *terminal moraines* are not so clear as might be desired; but glaciated *forms* are unmistakably observable on the walls of the valley in many places, and in some places even to the brim.

In order to understand why there has been so much doubt as to the former existence of glaciers in the lower valleys of the Sierras, it must be remembered in the first place that the granite in this region decomposes very rapidly; and in the second place that an immense period of time separates the epoch of the great glaciers such as that of which we are now speaking, and that of the smaller glaciers which occupied only the upper portion of the higher valleys. It is in these upper valleys only that glaciated surfaces are very conspicuous. And yet even there, these surfaces, although so comparatively recent, are only found in patches. A few hundred or thousand years will probably remove every vestige of them, except where protected by soil. Glaciated *forms*, on the contrary, are much more permanent; but these are most conspicuous always in the bed of the glacier; and there they are apt to be covered up, after the retreat of the glacier, by lake deposits and by debris brought by tributary glaciers coming down through side cañons. In the Yosemite the glacial bed rock has been entirely covered up by these means, particularly by *lake deposits*, which are here very conspicuous. So far as glaciated *forms* and surfaces are concerned, therefore, only the walls remain to tell the story.

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* Vol. 1, p. 422 and seq.

† p. 73.

Mr. Muir first drew my attention to the fact that the almost perpendicular walls of Yosemite are in many places absolutely *free from talus*, the hard, smooth rocky wall coming down directly to the level meadow soil of the valley. In every such place the wall rock is found to be very hard and solid, more or less projecting and rounded, with a gentle slope on the side looking up the valley, and with a more abrupt curve or even broken on the lower side; in other words it is a true *moutonnée* form. Other parts of the wall are irregular and destitute of glacial signs; but the reason is evident in the piles of debris at the bottom. The former consist of hard material, little affected by joints, and have, therefore, remained in the condition in which the glacier left them; the latter consist of softer materials, and have crumbled and thus destroyed the glacial signs since the period of glacial occupation. Thus, on the north side of the valley every projecting shoulder is rounded and smooth and without talus, while every re-entering angle is rough and jagged, and has its pile of debris at its base. In some cases, as for example high up on Washington column, the smoothness of these projecting shoulders is such that they glisten in the sunshine. I believe if these spots could be reached they would be found scratched and polished.

These *moutonnée* forms are found principally on the north wall. The reason is evident. In an east and west chasm so deep, so narrow, and with walls so perpendicular as this, the sun shines with full effect only on the northern wall. Therefore ice and snow and small tributary glaciers hung about the cliffs and side-cañons, on the south side, long after the retreat of the main glacier, and after the northern wall was entirely clear. Under the disintegrating influence of this frost, the glaciated forms and surfaces produced by the main glacier have been entirely destroyed on the southern wall.

Again: although the old terminal moranies are not so clear as might be desired, yet I think no one who was examined the glacial valleys of the Sierras, and thereby made himself thoroughly familiar with the appearance of half-effaced moraines, can fail to perceive them in Yosemite. The bottom of this valley, as of all the glacial valleys of the Sierras, consists of a succession of level meadows covered with grass and flowers, separated by higher ground overgrown with pines. The soil of the meadows is *stratified sands and gravels*, while that of the higher ground is composed of boulders of all sizes mixed with finer material without sorting—in other words, of *moraine matter*. These boulder piles are certainly terminal moraines, and the meadows are ancient lakes or marshes, produced by the damming up of the river waters by successive terminal moraines left by the retreat of the glacier and afterward filled up by river silt.

Tributaries of Merced Glacier.—The north fork of the Merced river now drains the whole area bounded by Mt. Hoffman, Cathedral Peak, Mt. Lyell and Mt. Clark. (See map.) Its tributaries are Yosemite creek coming from Mt. Hoffman, Tenaya creek from Cathedral Peak, and other peaks in the vicinity, the main Merced coming from Mt. Lyell group, and Illiluette from Mt. Clark group. In glacial times precisely the same basin was drained by the *Merced glacier*, and there was a tributary *glacier* corresponding to each of these tributary *rivers*. Only two of these I have personally examined. Mr. Muir has examined them all.

(a.) *Little Yosemite Glacier.*—The most important of these tributaries, in fact the main feeder of the Merced glacier, was formed by ice streams coming from Cathedral peak, and especially from Mt. Lyell group, which, uniting in Little Yosemite valley, passed down as ice cascades over the Nevada falls and the Vernal falls, and so joined the other tributaries, especially the Tenaya glacier, to form the great glacier which filled Yosemite. At the point of former junction with the Tenaya, as first pointed out by Mr. King, a median moraine is still traceable.

Our route, as already stated, passed up the Nevada cañon to the top of the Nevada falls; thence along the rim of Little Yosemite valley, and thence over a ridge into Feldspar valley (F. V.), and up this valley to Cathedral peak. Glaciated surfaces and erratics were found nearly all the way. As we passed along the edge of the rim of Little Yosemite, we had a magnificent bird's-eye view of the wonderful dome-like form of nearly all the prominent points about this valley. Standing thus above, and looking down upon them, their striking resemblances to *glaciated forms* cannot be overlooked. The whole surface of the country is *moutonnée* on a huge scale. If so, then the greater domes about the Yosemite have been formed in a similar manner. If so, then the whole surface of this region, with its greater and smaller domes, has been moulded beneath an universal ice-sheet, or confluent glacier which moved onward with a steady current careless of domes. The period of this ice-sheet, of course, preceded that of the separate glaciers which I am now tracing.

Another point of some interest which I observed here was the existence, everywhere in the lower part of Little Yosemite, even down to the Nevada falls, of erratics of a peculiar granite composed of very large and very perfect crystals of feldspar, cemented together by a paste of finer granite. These crystals, which were often four or five inches in length, stood out on the weathered surfaces so that the boulders looked like masses of coarse conglomerate or breccia. We traced these boulders up Feldspar valley (so-called from this remarkable granite) to their parent rock, viz: Cathedral peak and other peaks and comb-like ridges in that vicinity.

Cathedral Peak (11,000 feet high) is the most conspicuous of a cluster of sharp spire-like peaks and comb-like ridges in this vicinity; spires and ridges so steep and sharp that it seems difficult to understand how they can remain standing from year to year. These seem all to be formed of the peculiar granite spoken of. From the immense masses of snow which once accumulated amongst these peaks, and which still linger there in small quantities in sheltered spots, in glacial times there issued a glacier which passed down Feldspar valley and, joining the Little Yosemite glacier, poured its icy flood over the Nevada and Vernal falls into the Yosemite valley.

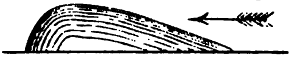
(b.) *Tenaya Glacier.*—From the same group of peaks emerged another glacier, which flowed farther west into the valley of the Tenaya fork. This tributary glacier was there joined by a stream of ice coming from the overflow of the Tuolumne glacier at D, as already pointed out by Whitney; and the combined streams flowed down the Tenaya cañon into the Yosemite valley. In the upper track of this glacier, especially of the tributary which emerged from the Cathedral peaks, I found the most beautiful examples of glaciated surfaces I have ever seen; rock basins scooped out by the glacier and now filled with pure water, forming exquisite little lakes; acres of glaciated surfaces, so smooth that it was difficult to ride over them. These glaciated surfaces were the more beautiful on account of the large feldspar crystals contained in the granite. A glaciated surface had the appearance of polished brecciated marble. Just above Lake Tenaya stands a mass of granite 800 feet high and half a mile or more long, directly in the track of the glacier. It has been polished on every side and over the top until it is rounded like the carapace of a tortoise.

The Tenaya cañon between Lake Tenaya and Mirror lake I have not examined. Just below Mirror lake there is a pile of boulders across the cañon. This I believe to be a moraine.

Below this and before reaching Yosemite proper, there is a succession of small meadows, three in number, separated by debris piles. These have been formed, I think, in the same manner as Mirror lake, viz., by water accumulated behind moraines left by the retreating glacier.

II. Tuolumne Glacier.

Whitney has already pointed out the fact, that at one time a great glacier filled the Tuolumne meadows, and after overflowing its banks at the divide between this valley and the basin of the Merced, and sending a branch down the Tenaya cañon, as already explained, passed down the course of the Tuolumne. One of the moraines left by its subsequent retreat is seen a few miles below Soda Springs. This glacier, according to Whitney, probably passed into, filled and passed beyond, Hetch-hetchy valley. Traced from its source in Mt. Lyell to the mouth of the Hetch-hetchy valley, this glacier could not have been less than forty miles long.

Tributaries.—The great Tuolumne glacier had many tributaries, three of which I have personally examined. One of these may be traced by a succession of meadows and other glacial signs up to Mt. Dana and Mt. Gibbs, a second in a similar manner to Mono pass, while the main branch is very clearly traceable up the Tuolumne meadows to Mt. Lyell and Mt. McClure. All of these tributaries met above Soda Springs. At this point, directly in the path of the combined glacier, stand two rocky knobs 500 to 800 feet high. They are smoothed and rounded on every side and over top, more sloping on the upper or eastern side, and more abrupt on the lower or western side, thus:  In a word they are perfect examples of *moutonnée* forms on a grand scale. But what interested me far more than anything else, was that the main branch of the Tuolumne glacier, far up among the cliffs and peaks of Mt. Lyell, *still exists as a living glacier*, in a feeble state of vitality it is true but certainly living. My attention was first directed to this fact by Mr. Muir.* and I visited Mt. Lyell with him for the purpose of convincing myself of so interesting a fact.

The summits of Mt. Lyell (13,300 feet high) and Mt. McClure consist of a vast amphitheater, irregularly circular in form and several miles in diameter, surrounded by almost perpendicular cliffs with sharp jagged crests. In this circular amphitheater there still exist extensive snow-fields, no doubt of considerable thickness. In glacial times this great amphitheater was completely filled to the brim with snow. It was the womb in which originated and from which emerged the Tuolumne glacier. From the immense mass of snow accumulated here, the glacier poured its icy flood down the Tuolumne meadows to Soda Springs, where, joined by other tributaries, the swollen stream overflowed its banks and poured a portion of its flood over the divide into the Tenaya cañon, while the greater portion went down the Tuolumne valley to, and beyond Hetch-hetchy; how far is not known. As the Glacial period passed away, the point of the Tuolumne glacier receded again, leaving terminal moraines here and there in the course of its retreat. When the point passed again the place of confluence above Soda Springs, the tributaries separated and retreated each its several way to the snow fields above. This retreat has been already completed; the points of the glaciers have been retired within the snow fields; these snow fields consist wholly of snow and névé and not, as far as can be seen, of glacier-ice. Nevertheless, in the case of the snow-remnant of the main branch of the Tuolumne glacier in the Mt. Lyell amphitheater, *true glacial motion still continues.*

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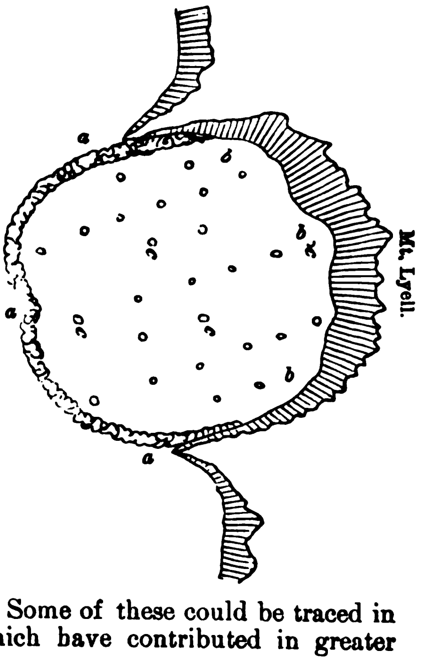
* See this Journal, for January, vol. v, p. 69.

The evidences of this important fact are abundant. The whole irregular amphitheater formed by Mt. Lyell, Mt. McClure and other unnamed summits, is not now filled with snow, but only the more sheltered *coves*. One of these, viz., that formed by Mt. Lyell, I have rudely represented in the accompanying figure. The mass of snow occupying this cove is about a mile in length, and about half a mile wide. Now, along the lower margin of this snow field and closely in contact with it, there is as perfect a *terminal moraine* as can be imagined. It is an irregularly crescentic line of boulders *a a a*, forming a pile 20 feet high, 50 feet wide at base, and about a mile long. On the surface of the snow also may be seen scattered blocks of stone *c c c*, some of them of great size, which have fallen from the perpendicular cliffs *b b b*, and are on their way to join the terminal moraine below. Some of these could be traced in lines to points of the cliff which have contributed in greater abundance.



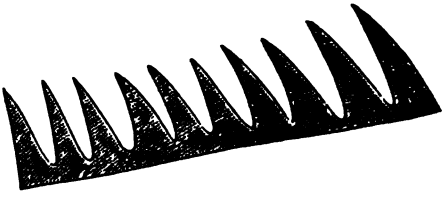
Between the cliff *b b b* and the snow, there is an empty space like a crevasse, 4 to 5 feet wide, evidently produced by the tearing away of the moving snow from the perpendicular cliffs. In the language of Alpine travelers, it is a *bergschrund*. Mr. Muir has also found a crevasse several feet wide in a similar snow field on Mt. McClure.

After examining the snow field of Mt. Lyell, our party left for Mono lake. Mr. Muir and Mr. Clarke remained, with the intention of re-ascending either Mt. Lyell, or Mt. McClure, and driving stakes in a line across the snow for the purpose of determining the rate of motion by subsequent examination. After my return to Oakland I learned from Mr. Muir the result of these experiments. When examined again at the end of 46 days, stake No. 1 had moved 11 inches, No. 2 18 inches, No. 3 34 inches, and No. 4 which was near the middle, 47 inches.* Both the fact of *motion* and its *differential character* are therefore certain.

Here, then, on Mt. Lyell we have *now existing*, *not a true glacier* perhaps, certainly *not a typical glacier* (since there is no true glacier-ice *visible*, but only snow and névé; and certainly *no protrusion of an icy tongue beyond the snow field*); yet, nevertheless, *in some sense a glacier*, since there is true differential motion and a well-marked terminal moraine. It is in fact a glacier in its feeble old age—feeble remnants of the great Tuolumne glacier—a glacier once of grand proportions and playing an important part in mountain sculpture, but now in its *second childhood*.

Mr. Muir tells me that he has found, hiding away among the highest peaks of the Sierras, several elderly glaciers in a similar sad condition.

Before leaving the subject of Mt. Lyell glacier (if I might so call it), there is another point to which I would call the attention of physicists. The surface of the snow of this great snow field I found traversed, in a direction at right angles to the slope, by *sharp blades* of half compacted ice, about two feet apart and two, three, four or even five feet high, so that a section in the direction of slope would be somewhat like the adjoining figure. Climbing up the slope over these ice blades was certainly the most difficult climbing I have ever attempted. Stepping from blade to blade, was attended with great risk of repeated



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* An account of these experiments, taken from the Overland Monthly, will be found in this Journal for Jan., vol. v, p. 69.

and painful falls. The crests of these blades were not continuous, but irregular both in outline and in trend; very much in this respect like ripple-marks or like waves. Again, the irregularities in successive blades or waves were so related to each other, as to give rise to lighter *lines of slope* distinctly visible at a distance. Thus viewed the two sets of lines were related to each other like the lines of an engraved map, the blades representing the *level lines*, and the lighter lines the lines of slope, so that a surveyor had only to copy these in order to make a perfect map.

I was for some time at a loss to account for these blades. I at first thought they were in some way connected with the motion of the glacier, and might be transverse crevasses sharpened by action of the sun. But I now believe that the true explanation was given me a few weeks ago, in conversation, by Mr. J. T. Gardiner, formerly of the Geological Survey. According to Mr. Gardiner, the transverse ridges or blades are produced by the *action of sun on wind ripples.* During the winter the wind blows mainly down the cañon and the loose snow is drifted into wind-ripples; during the summer, when neither rain nor snow falls for many months, the snow is greatly wasted, but more in the troughs than on the crests, on account of the reverberation of heat within the troughs. The lighter lines were, of course, in the direction of the wind. Whether or not *sand ripples* produced by wind or by currents exhibit also lighter lines in the direction of the current, I do not know.

III. BLOODY CAÑON GLACIER.

The Sierras on their western side slope gradually for 50 or 60 miles; but on the eastern side they are very precipitous, so that the plains, 5000 to 7000 feet below the crests, are reached in two or three miles. In glacial times, long complicated glaciers, with many tributaries, occupied the western slope, while on the eastern slope innumerable short, simple glaciers flowed in parallel streams down the steep incline and out, for several miles, on the level plain. The only one I have carefully examined is that which occupied Bloody Cañon. This glacier has been mentioned by Whitney, but there are several points of interest which are not mentioned in his very brief sketch.

Mono pass is a watershed from which runs a stream in either direction. In glacial times it was a *snowshed*, from which issued a *glacier* in either direction. One of these flowed westward, and formed a tributary of Tuolumne glacier, as already explained, while the other flowed eastward down the steep incline of Bloody Cañon to the level plains of Mono and out on these plains for several miles. I know no place where the formation of glacial lakes can be so well studied.

Bloody Cañon descends about 4000 feet in two or three miles. The glaciation of the walls and bottom of this steep, narrow, rocky gorge is extremely beautiful. About one-third of the way down, just where the steep declivity becomes nearly level, only to plunge downward again, occurs an exquisitely beautiful lake. Its borders, especially on the upper and lower sides, are polished and scored in the most perfect manner, and its bottom so far as could be seen is also smooth and rocky. Without doubt this is a *pure rock basin scooped out by the glacier* at this place, on account of the change of inclination here.

When the tourist has reached the level plains, after he has escaped from the rocky walls of the cañon, he still finds himself confined between two *parallel ridges*, composed entirely of boulders and earth, at least 500 feet high, only half a mile apart, and extending five or six miles out on the plains. These parallel ridges are evidently the *huge lateral moraines of Bloody Cañon glacier.* Just where the level plain, and, therefore, the debris ridges, commence, there is a lake of considerable size, occupying nearly the whole space between the ridges. Immediately below this lake, the ridges are seen to send off each a branch ridge, which, curving toward each other, meet in the middle. This crescentic transverse ridge connecting with the main lateral ridges is evidently a terminal moraine, behind which the lake has accumulated. Through this, of course, the stream has cut its way. Beyond this terminal moraine there is a *marsh.* Below this the lateral moraines are again seen to send off each a branch which, curving, again meet. It is another terminal moraine, behind which was probably formed first a shallow lake, which has been gradually filled up with sediment and converted into a marsh. Beyond the marsh is a *meadow* which has been evidently formed in the same way, for below the meadow there are evidences of still another terminal moraine.

From the summit of any of the volcanoes near by on the plains, a fine bird's-eye view of all I have described is obtained. The long lateral moraines and the three or more terminal moraines separating the lake, marsh, and meadow, are seen at one glance. In addition, many other *similar* ridges are seen stretching out on the plains from the gorges of the mountains, some of far greater height and length than those described. They are all no doubt moraines, but I have only observed them at a distance.

Whitney, in the first volume of the Geological Survey of California (p. 437), speaks of the lakes in Bloody Cañon, but ascribes them all to the collection of water behind terminal moraines. But it is evident that the *upper one* is a pure rock basin; its lower rim is not a terminal moraine, but is a beautifully glaciated rock. In fact, we have here admirable examples of the two kinds of glacial lakes; those of one kind are accumulated in *rock basins* scooped out by glacial erosion; those of the other kind are accumulated behind terminal moraines; the former are found only in the higher slopes, the latter are usually found lower down. These latter graduate insensibly into marshes and meadows. Standing on the top of Mt. Dana on my previous trip, I am sure more than 50 of these lakes and marshes could be counted.

After leaving Mono lake, our party traveled northward among the foothills on the east side of the crests of the Sierras. We were too far from the crests to observe any glaciated surfaces or glaciated forms. Until we approached Lake Tahoe, I saw no unmistakable signs of glacial action, except here and there what seemed to be long hills of debris brought down from the Sierras. Nevertheless, the sharp, jagged, saw-like crests of the Sierras seen in the distance, with their palisade-like cliffs and circular amphitheaters still filled with snow, clearly showed that these were the deserted homes of many former glaciers.

IV. Carson Cañon Glacier.

Hope valley and Lake valley (Lake Tahoe valley) lie countersunk upon the very top of the Sierras and in the direction of the chain. This chain, therefore, is divided at this point into *two crests*, east and west of these valleys. From the plains of Carson, on the east side of the Sierras, our party passed westward through Carson Cañon, a magnificent gorge which cuts through the eastern crest into Hope valley. Throughout this cañon I observed the clearest evidences of glacial action, in the form of scored, polished and *moutonnée* surfaces. In Hope valley the same evidences were abundant. From the direction of the scratches, it was evident that a glacier once came from the south down Hope valley, and turned at right angles and passed out by Carson Cañon. Looking southward from Hope valley, a cluster of snowy peaks was distinctly visible about 15 or 20 miles distant; from these no doubt came the glacier. I did not trace this glacier to its source; but Mr. Hawkins, a student of the University and one of our party, who lives in this vicinity, and has been thoroughly educated in the observation of glacial signs, subsequently went up this and other valleys to the southward and examined them carefully. To the south of Hope valley is Faith valley, and to the south of this Charity valley These three are separated from each other only by low rocky ridges. These ridges are polished, scored and moutonnéed in the most perfect manner; they are, in fact, the lips of consecutive lake basins scooped out by a glacier.

It is perfectly evident from the direction of the scorings observed by myself and by Mr. Hawkins, that a great glacier came down from the snowy peaks to the southward, and, gathering tributaries from the peaks on either side but especially from the eastern crest, filled the whole basin of the three valleys, forming an immense *mer de glace* 15 miles long and 3 or 4 miles wide, which, being blocked to the northward by high mountains, themselves probably contributing their share of ice, turned at right angles and escaped eastward to the plains of Carson, by the deep narrow gorge of Carson cañon.

V. LAKE VALLEY GLACIER.

From Hope valley our party passed westward over a low ridge of debris, then through a cross valley nearly filled with a shallow lake and its surrounding marsh and meadow, and then down into Lake valley. The ridge of debris is about 600 feet above Hope valley, and 100 feet above the cross valley. Although I could not find the clear evidences of glaciated surfaces, yet I feel almost sure that the Hope valley *mer de glace* found a second outlet *to the west* through this cross valley into Lake valley. The debris pile spoken of is, I think, a lateral moraine of Hope valley glacier, formed at a later period, when this higher outlet had dried up.

Lake valley heads apparently in the same region of snowy summits as Hope valley. The two valleys run nearly parallel to each other, but Lake valley extends much farther northward, and is lower by several hundred feet. In this valley I found again evidences, which I could not misunderstand, of the former presence of a glacier. The glaciated surfaces were not so clear as in Hope valley and Carson cañon, but the rounded forms of the nearly perpendicular projecting rocky shoulders on each side of the valley were unmistakably due to ice. I have no doubt that well marked glaciated surfaces would be found in abundance higher up the valley, or by diligent search even in the region I passed over. Evidences of the main glacier, however, became less and less clear, as we passed down toward Lake Tahoe; being obscured partly by the more recent action of tributary glaciers coming down the side cañons, and partly, as we approached the lake, by what I take to be lake deposits, indicating a former higher and much more extensive condition of the lake. I conclude, from what I observed, that at one time a great glacier came down from the south, filling the whole Lake valley for 20 miles, then filling the great basin of Lake Tahoe 25 miles long, 15 miles wide, and 1600 feet deep, and finally escaped northward and eastward by the Truckee cañon. Both Lake valley and Lake Tahoe are flanked on either side by high mountains: this great *mer de glace* therefore received tributaries at every step.

VI. AMERICAN RIVER GLACIER.

On returning homeward we retraced our steps southward up Lake valley for about 8 miles, then crossed the western crest of the Sierras by Johnson's pass, and then went down the cañon of the south fork of the American river.

Among the tributaries which flowed into Lake valley glacier, one of the largest came down from Johnson's pass. The scored, polished, *moutonnéed* surfaces were very conspicuous and beautiful. From the same pass another much greater glacier flowed westward and southward down the grand cañon of the south fork of the American river, for at least 25 miles. For this distance glaciated surfaces and glaciated forms are distinct, and in many places very fine. For example, at Sugar Loaf, about 20 miles down from the pass, the gorge becomes very narrow and precipitous, with perpendicular rock walls which almost rival in grandeur the cliffs of Yosemite. Through this narrow gorge the glacier was squeezed. The effects of glacial erosion are seen on the sides, but especially on several rocky prominences which stand in the middle of the gorge. These have been smoothed and scored on every side in the most perfect manner. One of these is the "*Sugar-loaf rock*," from which the village takes its name.

Four or five miles below this point, glacial marks disappear, and the cañon changes its character. The upper part of the cañon consists of a succession of broad level *meadows;* the lower part is sharply V-shaped, and contains no longer any meadows. The change marks, I believe, the distinction between *glacial* and *river erosion.* This change, however, marks also the change from granite to slate. Glaciers may have passed still lower down the cañon; but if so the glacial form of the cañon has been greatly modified by water-erosion, which has cut far below the glacial bed.

VII. SOME GENERAL REFLECTIONS.

A. *General Ice-sheet in California.*—It is generally admitted that during the glacial epoch the polar ice-cap extended southward in North America, at least as far as 40° N. lat. This universal ice-sheet flowed southward and eastward on the Atlantic slope—southward in the middle region of the continent, and probably southward and westward on the Pacific slope, in Oregon, and Washington territory: in other words, its general direction was southward and seaward. We have given 40° as about the southern margin; but from this southern margin the general ice-sheet stretched out finger-like prolongations still farther south down the valleys, in the form of *separate* glaciers. The position of many of these separate glaciers have been traced by eastern observers. There can be no doubt, however, that, favored by the great altitude of the Sierras, the universal ice-sheet itself was extended along this chain at least to Middle California, lat. 37°, and probably even to Southern California, lat. 33° or 34°. The flow of the ice-sheet in the Sierras was probably determined by the slope of this great chain, rather than by the arctic elevation; it was eastward and westward from the crest, rather than southward along the range. This general ice-sheet stretched out finger-like projections eastward and westward in the form of separate glaciers, far down the lower valleys.

The former existence of such a flowing ice-sheet on the Sierras is, I believe, proved by the domes and dome-like forms so abundant on the higher slopes of the Sierras, especially about Yosemite. These are the *roches moutonnées* of an ice-sheet which not only filled the cañons, but covered the peaks and domes—moulding itself upon their surfaces and determining their forms. While the great mass of this enormously thick sheet flowed in one general direction with a steady current, its lower portions, or portions in contact with the earth, doubtless conformed more or less to the greater valleys.

The actual forms of a glaciated surface are determined not only by the nature of the eroding agent, but also by the character of the material eroded. This latter factor is extremely variable. Sometimes it is the dip of strata or of cleavage, sometimes relative hardness in different parts, sometimes it is other kinds of structure, which have the controlling influence. The slopes of the Sierras, where the general erosion cannot be less than several thousand feet vertical, afford an excellent opportunity of observing how resulting forms have been determined by the rock structure.

According to my observations, there are two striking peculiarities in the structure of the granite about Yosemite: one is a *concentric structure*, on a huge scale; the other is a coarse *perpendicular cleavage*. The former is beautifully seen in the Royal Arches, which consists of several concentric semi-circular division-lines or joints, of nearly 2000 feet radius, seen on the perpendicular walls of Yosemite: this structure gives rise to the domes and dome-like forms so prevalent in this region. The other gives rise to the *perpendicular cliffs* and spire-like forms so common in the same region. It is idle to speculate on the origin of Yosemite and of the domes in its vicinity, without taking this peculiar structure into account. The origin of Yosemite we will discuss presently. The dome-like forms are, it seems to me, the combined result of ice erosion and the concentric structure. Water-erosion, it is true, if sufficiently great, would probably have brought out these forms, but ice-erosion would do so *more perfectly*, both because it is a more powerful agent and because it tends, under all circumstances, to determine forms of this kind.

As the glacial period waned, the universal snow sheet retreated to the summits, and the slopes of the Sierras were now occupied, east and west, by great but *separate* glaciers, several of which I have attempted to describe. These in their turn retreated upward and took refuge in the snow fields, nestled amongst the peaks and amphitheaters of the highest summits. There, their still remaining but feeble existence has escaped observation, until revealed by the indefatigable industry and keen scrutiny of Mr. Muir.

During the fullness of the glacial period, I suppose glacial or *moutonnéed* forms covered every portion of the Sierras, even to the summits. These forms have not been retained on the summits, because they have been subsequently modified by continuance of snow in this region. The evidences of *universal* glaciation, therefore, must be looked for only on the higher slopes.

B. *Origin of Yosemite.*—Whitney thinks* Yosemite was formed by sudden *engulfment* of a portion of the slope of the Sierra: he believes that it is impossible otherwise to account for its nearly vertical walls. My own observations, on the contrary, lead me to believe that it has been produced by glacial erosion. My reasons for believing so are briefly as follows:

It is a remarkable fact that many of the great glacial valleys of the Sierras become deep narrow cañons with precipitous walls *near the junction of the granite with the slates* which lie on the *lower* slopes. This is the position of Yosemite. Similarly the Tuolumne glacier valley, near the junction of granite with the slate, becomes Hetch-hetchy, a valley very similar to Yosemite, and almost rivaling it in grandeur. Again, the valley of the American river becomes very deep, narrow, and precipitous just before reaching the slate. How general this is I do not know, but the fact that all these cañons, with nearly vertical walls, have been *occupied* by glaciers, makes it almost certain that they have all been *formed* by this agency. If Yosemite were unique, we might suppose that it was formed by violent cataclysm: but *Yosemite is not unique* in *form*, and therefore probably not in *origin*. There are many Yosemites. It is more philosophic to account for them by the *regular* operation of known causes. I must believe that all these deep perpendicular *slots* have been sawn out by the action of glaciers; the *peculiar verticality of the walls having been determined by the perpendicular cleavage structure* before spoken of.

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* Geol. Survey, vol. i, p. 421. Yosemite Guide Book, p. 74.

C. *General structure of the Sierras.*

I have long believed that granite is but the last term of metamorphism of siliceous rocks,* and that metamorphism only occurs in deeply buried sediments, and therefore that where granite appears on the surface, as it so often does in great mountain chains, it has been exposed by immense erosion. It seems to me this is very clearly shown in the Sierras. There can be no doubt that the *slates* which now form a belt on lower slopes of this chain, *once covered the whole chain*, and have been removed by erosion. Along the very crest the slate still remains in patches, forming the highest peaks; such as Mt. Dana, Mt. Lyell, and many others. This crest, forming the divide of the snow-sheet, has been less eroded, and the slate therefore still remains, but on the higher slopes where the glacial erosion was universal and very great, the slate has been nearly all cut away and the granite left bare. A few patches of slate are left, however, even on these slopes to tell the story; such a patch is found, for example, on Mt. Hoffman. Only on the lower slopes, near the *limits of the ice-sheet*, does the slate appear again in force, as the auriferous slate belt. The limit of the granite region marks, I believe, the limit of the ice sheet.

The actual amount of general erosion it is perhaps impossible to know, but it has been evidently enormous. I think there is good reason to believe that in addition to the whole thickness of the slates, several thousand feet of underlying granite has been also removed. The highest peaks of the Sierras are composed of slates, but the valleys even in this region are composed of granite: the erosion here, therefore, has been through the slate into the granite. A little lower down, the Cathedral and other peaks and ridges in the vicinity are composed of very coarse and very feldspathic granite: the erosion here has been through this at least 2000 feet and down to the harder and more siliceous granite. Still lower down, about Yosemite, only this harder granite exists; both the slate and the softer granite has been entirely removed.

D. *Ice-erosion and water-erosion compared.*

The *great bulge* of the earth's crust which constitutes a mountain chain, is doubtless produced by general causes affecting the whole earth, probably by shrinkage of the interior more than the exterior, by which the face of the old earth becomes wrinkled: but the *smaller inequalities* are almost always produced entirely by *erosion*. How any one who has ever been amongst the high Sierras can for a moment doubt this fact, I cannot understand. Standing amongst these mountains, all that constitutes scenery—every peak, ridge or dome; every valley or cañon—is evidently due to this cause alone, except in those parts where recent volcanic action has taken place.

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* On Great Features of Earth's Surface, this Jour., vol. iv, p. 467.

Now the forms produced by erosion depend partly upon the *kind of rock*, and partly upon the *kind of erosion*. The forms determined by *water* are different from those determined by *ice*. Standing in the middle of the San Joaquin plains, on a clear day, the crests of the Sierras are seen on one side and of the Coast range on the other. Nothing can be more striking than the contrast between the two; the sharp saw-like, teeth-like outline of the former, and the rounded outlines of the latter. The reason is that the one has been determined by the action of snow and frost; the other by water only. The contrast is still more conspicuous when we are among these summits. In all the region where perpetual snow still lingers, the Sierras are studded with peaks, and spires and comb-like ridges, so sharp that they seem ready to fall over; and especially with great amphitheaters, bounded on several sides by almost perpendicular palisade-like walls, with sharp serrated edges.* These great amphitheaters are the wombs of ancient glaciers and are still filled with snow.

Such is the contrast in the summits. On the higher slopes the contrast is equally striking. During the period of the *ice sheet*, probably all the Sierra slopes, even to the summits themselves, were covered with rounded *moutonneé* forms on a grand scale. Subsequent action of ice and snow and frost and rivers have destroyed these, except in some localities. I am not now, however, speaking of the forms determined by the ice-flood, but of those determined since by *ice-streams* and *snow-fountains*. Water tends to form deep V-shaped cañons, while ice produces broad valleys with lakes and meadows. A camping party in the Sierras is made painfully aware of having passed beyond the limits of ancient glaciers by the sudden and entire disappearance of meadows and therefore of grazing for horses. Speaking in a very general way, water, it seems to me, tends to produce forms like fig. 1, while ice in the form of glaciers and eternal snow tends to the reverse forms of fig. 2. I know not how general these distinctions may be, but certainly the Coast range of this State is characterized by rounded summits and ridges, and deep

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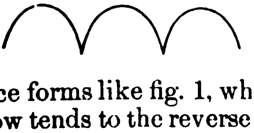

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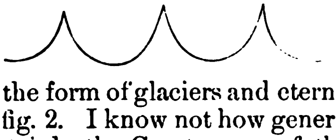

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* These amphitheaters occur, I believe, only in the *slate* of the summits. The tendency to *perpendicular cleavage* is no doubt, therefore, a necessary coöperating cause of this form: but the sharp peaks and spires about Cathedral peak are all granite.

V-shaped cañons, while the high Sierras are characterized on the contrary by sharp, spire-like, comb-like summits, and broad valleys; and this difference I am convinced is due in part at least to the action of water on the one hand, and of ice on the other.

ART. XXXVII.—*Meteorological effects upon the heights of the Tides;* by WM. FERREL. Letter to Professor BENJAMIN PEIRCE, Superintendent U. S. Coast Survey. (Communicated by the author with the authority of the Superintendent of the Coast Survey.)

CAMBRIDGE, MASS., Jan. 2, 1873.

Dear Sir: I have the honor to submit the following preliminary report of the principal results obtained from a discussion of a series of six years of the tidal observations in Boston harbor, with regard to the meteorological effects upon the heights of the tides.

The heights of high and low water of the tides for six years, from 1856 to 1861 inclusive, were computed by the formulæ and tables given in a previous report, and the results compared with the observations and the residuals noted. From the meteorological records of Cambridge Observatory for the same years, the corresponding barometric pressures and the directions and forces of the winds were obtained, and collated with the residuals. The range of the barometer was then divided into seven parts, and all the barometric pressures belonging to each one of these divisions were grouped together, and likewise the corresponding tidal residuals, and the averages taken and compared. High and low waters were at first kept separate, but the results subsequently combined. The observations also when the pressure was increasing, and those when it was decreasing, were kept separate in order to determine whether there is any perceptible difference on account of the inertia and the friction of the water preventing it from assuming at once the condition of static equilibrium. The following table of combined results of high and low waters were thus obtained.

RISING BAROMETER.			FALLING BAROMETER.			AVERAGES.	
No. of Obs.	Barometric pressures.	Tidal Residuals in feet	No. of Obs.	Barometric pressures.	Tidal Residuals in feet.	Barometric pressures.	Tidal Residuals in feet.
158	29·500	+0·307	279	29·480	+0·320	29·487	+0·315
363	29·708	0·152	569	29·717	0·212	29·713	0.192
449	29·842	+0·050	519	29·843	0·082	29·843	0·067
662	29·943	−0·030	695	29·940	+0·035	29·942	+0·002
887	30·053	0·062	820	30·050	−0·035	30·052	−0·049
962	30·197	0·115	845	30·190	0·125	30·195	0·120
628	30·420	−0·260	472	30·415	−0·240	30·418	0·251

In taking the averages in the last columns regard was had to the number of observations. The average barometric pressure is 30·007 inches. If, therefore, we put p for the pressure belonging to the average of any of the groups of observations, we shall have for the correction of the height of the tide $(30·007-p)C$, in which C is a constant to be determined from observation. This expression put equal to each of the average residuals in the last column, reduced to inches, with the corresponding pressures or values of p, gives seven equations for determining the probable value of C. Giving the equations weight according to the number of observations, we thus get $C=7·2$.

By comparing the residuals of rising barometer with those of falling barometer in the preceding table, it is seen that there is a perceptible difference near mean barometer, and that consequently the sea-level when the barometer stands at the mean and is rising is a little lower than when it stands at the mean and is falling. This difference, however, is very small, and indicates that the sea, under the average rate of the change of barometric pressure, very nearly assumes the condition of static equilibrium.

The theoretical value of the constant C above is 13·56, this being the ratio between the densities of water and mercury. I am unable to explain why its value in Boston harbor is little more than half this value. There can be no sensible amount of error in the constant as determined from so many observations. Any error in the tidal formulæ and tables with which the heights of the tides were computed and the residuals used in the discussion obtained, cannot affect sensibly the result, since all such small inaccuracies are eliminated by the great number of observations used along with other abnormal disturbances which are much larger. The difference also between the preceding value obtained from observations and the theoretical value, cannot be accounted for by the effects of inertia and friction, as is shown by a comparison of the results belonging to rising and falling barometers.

The value of this constant, as determined for several ports, is also much greater. M. Dausy found that at Brest the ocean rises 0·223 of a meter for a depression of 0·0158 of a meter in the mercury. (*Connaisance du Temps*, 1834.) This gives 14·1 for the value of this constant at Brest. Mr. Bunt, also, from a discussion of the residuals between theory and observations for three years at Bristol, obtained 13·4 for the value of this constant. (*Eleventh Report of the British Association*, p. 31.) Lubbock found 11·1 for Liverpool, but at London he found that the water rises 6·3 inches for a depression of 0·90 of an inch of the mercury. (*Phil. Trans.*, 1836, p. 11.) This gives only seven for the value of the constant, which is nearly the same as the value obtained for Boston harbor.

In order to obtain the effect of the winds upon the heights of the tides, all the residuals for the six years belonging to each of the eight principal points of the compass were grouped together, and also the corresponding averages of the forces of the winds and of the barometric pressures. The forces of the wind in the observations were represented by the numbers 0, 1, 2, 3 and 4, 0 denoting a calm, and 4 the strongest winds recorded. The following results were thus obtained.

Wind.	No. of observations.	Average force of wind.	Barometric pressure.	Average residuals.	Corrected residuals.
N.	244	1·6	30·007 + ·005	+0·21	+0.21
N.E.	317	1·7	+ ·009	0·23	0.24
E.	274	1·3	+ ·055	0·04	0·07
S.E.	131	1·5	+ ·053	+0·02	+0·05
S.	165	1·8	− ·074	−0·03	−0·07
S.W.	796	1·5	− ·021	0·10	0·11
W.	677	1·6	− ·073	0·18	0·22
N.W.	527	1·6	+ ·001	0·11	−0·11
Calm.	946		+ ·051	0·01	+0·02

The number of observations in the second column denotes the relative frequency of the winds from the different points of the compass, and the third column gives the average force of each wind according to the scale which has been given. The fourth column gives the mean barometric pressure, and the pressure for each wind. It is seen that with winds from N.W. around to S.E. the barometer stands above the mean, and from S.E to N.W., below the mean. The tidal residuals it is seen follow very nearly the same law. The full effect of the winds, therefore, is not represented by the fifth column, since they are generally affected by the corresponding deviation of the barometer from the mean, and require a slight correction by the preceding formula with the constant as determined. The last column contains the corrected residuals, which show the effects of the different winds having the average force given in the third column.

It is pretty generally thought that the winds cause very considerable changes of the sea-level, but it is seen from the last column of the table above, that an average N.E. wind raises the sea-level only about three inches, and a S.W. wind depresses it not quite so much. If the numbers in the scale of forces represent the velocities of the winds, the elevating and depressing effect of the winds may not be proportional to the forces, so that a very strong wind, denoted by four in the scale, may raise or depress the sea-level three or four times as much as a wind of the average force, this depending upon the of friction between the wind and the water. Very strong ls, therefore, may change the sea-level in Boston harbor a or more, and this agrees well with individual observations. ibout 700 tidal residuals of high water throughout the year), obtained from a comparison of computation by the forse and tables, with observations, only 10 amount to as much ıne foot. If therefore, we, suppose that these residuals are to the effects of the winds only, and no part of them to ır disturbing causes, and to errors of the tidal formulæ and es, even upon this supposition we know by actual measurets with the tide-guage, that in the course of a whole year, sea-level of Boston harbor is not often changed by the ds as much as one foot. The popular impression of large ıges of sea-level, no doubt arises from making observations loping beaches where a small perpendicular change of level luces a very great apparent change.

.n important meteorological result is shown in the fourth .mn of the preceding table, which is that the barometer ing calms stands very near the maximum of all the aves of the winds from the different quarters. This indicates the winds generally are of a cyclonic character, prevailing tly in the interior of the cyclone where there is barometric ression, and that the calms are mostly in the external part re there is high barometer.

he following table of results, brought out in the discussion, vs the annual changes of the barometer. As the unreduced ervations were used, a correction for temperature in this case to be applied, to reduce the barometer to the mean of the to correspond with the preceding results in which the ave corresponds to the mean temperature, or to reduce them, ısual, to the temperature of freezing. In the following e the reduction is to freezing and for capilarity.

Month.	No. of observations.	Barometer.	Red. to 32° and for capilarity.	Corrected barometer.
		in.		in.
January	324	29·995	—·004	29·934 + ·057
February	310	30·007	·020	+ ·053
March	363	29·886	·043	—·091
April	347	29·957	.066	—·043
May	354	30·013	·090	—.011
June	361	29·961	·110	—·083
July	354	30·020	·121	—·035
August	377	30·023	·119	—·030
September	360	30·104	·104	+ ·066
October	358	30·058	·076	+ ·048
November	335	29·947	·046	+ ·067
December	320	30·055	—·020	+ ·001

he mean barometer at the height of 71 feet, and reduced to temperature of 32°, is 29·934 in. Adding 0·082 in. for the reduction to mean sea-level, we get 30·016 in. for the mean height of the barometer at the mean sea-level in Boston harbor.

The last column in the table shows that there is a very small term with an annual argument and a coefficient of about 0·05 in., making the barometric pressure a minimum about May and a maximum about November. The number of observtions was not sufficient to eliminate the accidental irregularities sufficiently to determine this small annual inequality very accurately, but it is evidently too small to account for much of the observed annual inequality in the mean sea-level.

The following table of results is obtained from classifying the observations of the wind according to their directions, for each of the four seasons and for the whole year.

Season.	N.		N.E.		E.		S.E.		S.		S.W.		W.		N.W.	
	Obs.	S.F.	Obs.	S.F.	Obs.	S.F.	Obs.	S.F.	obs.	S.F.	Obs.	S.F.	Obs.	S.F.	Obs.	S.F.
Winter	92	153	51	75	22	37	23	36	36	68	189	288	221	378	193	337
Spring	68	117	120	183	106	137	45	63	57	109	183	293	181	323	113	210
Summer	34	52	85	149	101	143	37	61	50	67	269	416	150	238	104	176
Autumn	60	97	73	129	51	82	31	48	43	75	188	309	167	275	156	264
Whole year	254	419	329	536	280	399	329	208	86	319	829	1306	719	1214	566	987

The number of observations denotes, also, the relative frequency of the winds from the different points of the horizon. It is seen that the predominating winds are from W. and S.W. during all seasons of the year. The numbers headed S. F. are the sums of the numbers in the observations denoting the forces of the wind. There is some uncertainty with regard to the scale used by Professor Bond in denoting the forces, but it is supposed to be the scale from 0 to 6, in which 0 denotes calm, and 6 a velocity of 85 miles per hour, the numbers representing the forces being nearly proportional to the velocities. At any rate the sums of the forces above may be regarded as representing the relative sums of the distances passed over within the limits of the errors of such observations. With a table of latitude and departure, therefore, we readily determine the relative distances passed over and the directions, for each season of the year and for the whole year. We thus get the following table of directions from which the wind blows and the relative distances traveled.

Winter	N.	78°	W.	726
Spring	N.	85	W.	386
Summer	S.	71	W.	383
Autumn	N.	84	W.	498
Whole year	N.	87	W.	1920

It is seen that during the winter the wind blows from a point 12° N. of W., but in the summer from a point 19° S. of W.

This difference is caused by the difference in the relative temperatures of the land and sea in the two seasons, and shows that the winds have slightly a monsoon character. During the spring and fall, when the relative temperatures are about the same, the winds blow very nearly from the same point, which corresponds very nearly with that of the resultant for the whole year, which is 3° N. of W. The atmosphere in the course of the whole year moves a little more south than north. Dividing 1920 by 3299, the whole number of observations, we get 0·58 for the average force in the direction of the resultant. This, by the supposed scale used corresponds to a velocity of about eight miles per hour, in a direction a little south of east.

Very respectfully yours,
WM. FERREL.

Professor Benjamin Peirce, Sup't U. S. Coast Survey.

ART. XXXVIII.—*On the Origin of Mountains;* by JAMES D. DANA.

THE remark, that in Professor Hall's theory of the origin of mountains the elevation of mountains is left out (cited disapprovingly by Prof. Hunt on page 266), was made by me in volume xlii, of the last series of this Journal (p. 210, 1866), and not without due consideration. The importance which some still attribute to this theory makes it desirable that its special points should be stated here with more fulness than they were in the notice referred to, and that they receive further consideration. The points in the theory, selected out and condensed in the statement from the Introduction to the third volume of Professor Hall's New York Paleontology (where alone they have been published), are the following:

1. The Paleozoic strata of the Appalachian region bear evidence that they were mostly of shallow water origin.

2. Their great thickness, consequently, was attained through a slowly progressing subsidence, the axis of which was in the direction of the Appalachian chain.

3. This slowly progressing subsidence was occasioned by the weight of the slowly and successively accumulated sediments. The memoir says (p. 69):

"When these [" accumulations of sedimentary matter"] are spread along a sea bottom, as originally in the line of the Appalachian chain, the first effect of this augmentation of matter would be to produce a yielding of the earth's crust beneath, and a gradual subsidence would be the consequence."

4. This subsidence produced, as one of its direct results, the extensive folds and faults of the strata characterizing the Appa-

lachian formations. The fact that the folds are generally steepest on the northwest side may also be thus accounted for.

5. The formation of the Appalachians (and so of all mountains) was dependent upon, and the height related to, the thickness of the sedimentary accumulations, of which they are made; and a mountain chain was not a possibility over the Mississippi basin, because there "the materials of accumulation were insufficient."

6. (*a*) The elevation of the Appalachian mountains was not a result of the process of accumulation, or of the subsidence. (*b*) The elevation of mountains is, "of continental, and not of local, origin; there is no more evidence of local elevation along the Appalachian chain than there is along the plateau in the west." Again, the chapter says (p. 96): "It is this ultimate rising of continental masses that I contend for, in opposition to special elevatory movement along the lines of mountain chains." (*c*) After a continental elevation, the mountain range received its present shape mainly through erosion.

7. Metamorphism requires first large accumulations of rock material; and it went forward in the Appalachian region in consequence of the subsidence. As to the causes, Professor Hall says (p. 77)—after alluding to the view of Babbage and Herschel respecting a regular increase of temperature below following an increase in thickness of surface accumulations—"Such an increase of temperature would be much less than that usually supposed necessary for producing metamorphism; and it is extremely doubtful if any portion now exposed to observation ever reached a temperature much above that of boiling water. We must, therefore, look to some other agency than heat* for the production of the phenomena [of metamorphism] witnessed; and it seems that the prime cause must have existed within the material itself; and that the entire change is due to motion, or fermentation and pressure, aided by a moderate increase of temperature, producing chemical change."

A. The last of these propositions is an expression of the opinion that, in some way not understood, the heat required for metamorphism was generated within the strata that were altered. The effect was restricted by Vose in his Orographic Geology to pressure; but, with each of these authors, this pressure was due to the progressing subsidence referred to in the third proposition.

B. The first two propositions have general acceptation among American geologists.

C. The third and fourth, which are fundamental in the theory, have been shown in my former paper to be, as I believe, physical impossibilities; and LeConte, in his recent article, has further enforced this opinion. The third assumes that the first 500 feet in depth of sediments would press down the crust 500 feet, and so on to the end; but no reason is given why sediments under water should have so immense gravitating power, when the crystalline rocks of the Adirondacks, piled to a height of some thousands of feet *above* the water, had a firm footing close along side of the subsiding region.

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* On page 87, Hall observes that the lower beds may be *softened* by the heat that is received from below in consequence of accumulation above; but this remark is introduced not to make the heat a source of metamorphism, but to give a reason for the rocks yielding and subsiding under accumulating deposits, notwithstanding the property of heat ordinarily to cause expansion.

But while the weight of accumulating sediments will not cause subsidence, a slow subsidence of a continental region has often been the occasion for thick accumulations of sediments.

D. The fifth proposition announces a relation between the height of the mountains formed over an area, and the thickness of the sedimentary accumulations there previously made, preparatory to the elevation; and it further makes the absence of mountains from a region a consequence of small accumulations.

The relation set forth is a true and important one if taken in the most general way; but the application of it as a strict ratio, or as a universal law, encounters many apparent exceptions. In the Green Mountains of Vermont, the latest rocks of which are Lower Silurian, the conformable Silurian beds constituting them are probably not over half the thickness of those of the Appalachian region in Pennsylvania and Virginia; and yet the average height is greater, although exposed to erosion for a vastly longer time. For most mountain regions we have not the facts needed for the comparison—the thickness of the sediments preceding their formation and the amount of erosion since undergone being alike unknown. In some cases of composite mountain masses, like that of the Rocky Mountains, the principle hardly admits of application at all. According to King and others there were 10,000 feet in thickness of Cretaceous deposits laid down in Utah, with coal beds in the upper part of the series proving that subsidence accompanied the deposition; but, while there is evidence of subsequent disturbance in the region, and of the elevation since of a large part of the Rocky Mountain chain, there is, I believe, none that the Utah Cretaceous was raised into mountains overtopping the older ridges of the summit. There are, besides, cases of low plains, like the part of the Patagonian Pampas bordering the Atlantic, where for all that science knows, or ever will know, there may be a great thickness of conformable beds beneath. Again, there are areas now rising, like the coast region of Sweden, where thick accumulations of Tertiary sediments, or of any others since the Paleozoic, are wanting. There are many cases where the highest summits of a region occur at the crossing of two lines of elevation, showing that force has confounded the ratio of height to thickness of sediments, if any such had existed there.

Still, it is evidently a common fact that where mountains have been raised, there, in general, thick accumulations of sediments were previously made; and conversely. But the principle properly stated is—Where mountain ranges have been raised, there, in general, great subsidences, giving opportunity for thick accumulations of sediments, previously occurred; and conversely.

Unless the third and sixth of the above propositions are true, mountains are absent from the Mississippi because no local elevation extensive enough took place there; and no elevation was made, and there were likewise only thin accumulations of sediments, because the region, owing to its interior position, was not within the range of the continental-border oscillations.

E. By the sixth proposition, the influence of erosion in shaping a mountain is recognized, but no provision is made for its elevation—this elevation apart from general a continental elevation being denied. Thus Professor Hall's theory is strictly a "theory of the origin of mountains with the elevation of mountains left out." It accounts for plications by simple subsidence, but supposes the continent to get up high some way—the way not considered—without other plications, or any local uplifts, and on a crust so flexible that it will sink a foot for every foot of sediment added to the surface. The world abounds in cases in which part of the sea-border deposits of a period are now but little away from the old level, while other portions are many thousands of feet above it: *e. g.*, the Cretaceous strata of the United States; and it contains examples of modern rising of land. No principle has been found to explain such facts except that of *local elevation.*

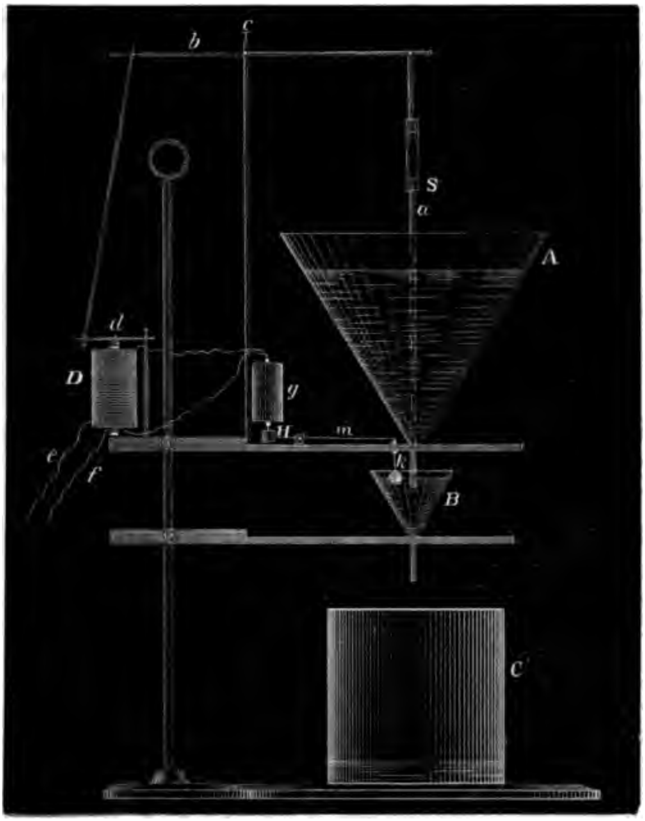
Since, then, the exposition which Professor Hall has made of his views offers nothing in explanation of the elevation of mountains—the event upon which the existence of any mountain depends—and since the only agent of change of level appealed to is one producing subsidence, and this will not work, so that there is no chance from it for the thick accumulations needed, or for the faintest plications or metamorphism, we may with reason pronounce the theory seriously deficient and defective.

ART. XXXIX.—*On an Automatic Filtering Apparatus;* by HARVEY W. WILEY, M.D., Professor of Chemistry, Indiana Medical College.

WHILE studying quantitative analysis in the laboratory of the Lawrence Scientific School, I was led to devise some plan by which time could be economized in filtering and washing precipitates.

Neither Mariotte's bottle nor the Bunsen pump were capable of general application.

As a result of a series of experiments the following apparatus was devised and successfully put into operation.



An ordinary filter-stand, with two arms, is fitted with a large glass funnel, A, of from one to four liters capacity. The lower arm holds a common small Bunsen funnel, B, so placed that the axes of the two funnels coincide. D is a small electro-magnet attached to the upper arm of the stand. The neck of the large funnel should be drawn out until it has an internal diameter of eight or nine millimeters. Passing down the center of the large funnel, and fitting into its neck is a glass rod, *a*, covered by a piece of a soft white caoutchouc connector. The glass rod is about twenty centimeters in length, and of such a size that when covered by the rubber hose it will completely close the orifice in the neck of the funnel.

This rod is connected by rubber with the lever, *b*, which is worked by the armature, *d*; *e* and *f* are two insulated copper wires connected with a small Grove, Bunsen or Daniel's cell; *e* passes through the magnet, *f* comes directly from the cell, and both at *g* are fastened into a small glass tube, the free ends denuded of their covering projecting three or four millimeters below.

The small funnel contains a glass bulb ten to fifteen millimeters in diameter, into which is soldered a small platinum wire. This wire is fastened to the long arm of the lever, *m*, the short arm of which carries a cup, H, holding a globule of mercury. The weight of the bulb, the amount of mercury, and the arms of the lever must be so adjusted, that when the small funnel contains no liquid the bob, *k*, will descend and the cup, H, rise, but when the small funnel is filling the relative weights of the two ends of the lever must allow the bob to rise with the liquid in the funnel B. This adjustment is easily effected by varying the amount of mercury in H. When the parts have all been properly connected, the glass rod, *a*, should exceed the weight of the armature, *d*, by such an amount as will enable it to thoroughly close, by its excess of weight, the neck of the funnel A. By means of the thumb screw, *c*, the lever, *b*, should be moved until the space between the armature and the poles of the magnet measures two or three millimeters. The small funnel, B, properly fitted with a filter paper, is fixed in the lower arm, which is raised and fastened in its place. The proper position for this lower arm is readily found by trial.

Let B be filled with distilled water as represented in the diagram. When the bob, *k*, stands as in the figure the mercury in H should not touch the copper wires. As the liquid runs out of B, the bob sinks, the cup rises until the wires touch the surface of the mercury. This immediately completes the electric circuit, the armature, *d*, is pulled down, the glass rod, *a*, elevated, and the liquid in A flows into B until the bob is raised to its first position. This breaks the circuit, the rod, *a*, falls, the flow of the liquid from A to B is stopped. After using distilled water for two or three minutes until everything is found properly adjusted, the substance to be filtered is poured into A, and no further attention is required until the whole of it has run through into B; *a* is then to be disjointed at *s*, and with the wash bottle, using *a* as a rubber, any precipitate adhering to A is easily washed through into B; B is then to be lowered and removed and washed.

Advantages of the Apparatus.

Washing by decantation.—The whole of the supernatant liquid can be poured at once into A. The beaker is refilled with the washing fluid, and by the time the first portion has run through you are again ready to decant. By other processes filtration has to be suspended each time until the supernatant liquid is clear.

Washing precipitate on filter.—By means of the thumb screw, *c*, the apparatus can be adjusted to discharge jets of water from A with force enough to thoroughly stir up the precipitate each time, thus greatly facilitating the process.

Saving time.—This is the chief feature of the automatic filter. I give below some tabulated data taken from a great number of observed filtrations. In all these cases the results were as good as those usually obtained by hand and in several instances, owing to special care, better.

I. Washing precipitate $BaSO_4$ by decantation.

Adjustment 5 min., filling 3 min., washing 12 min., total 20 min. Time apparatus at work, 3 h. 15 min. Amount wash water, 2 liters 600 $\overline{\text{c.m.}}^3$.

II. Washing precipitate $Fe_2O_6H_6$.

Adjustment apparatus 2 min., filling 3 min., washing 15 min., total 20 min. Time, apparatus at work, 2 h. 40 min. Amount filtrate 2 lit. 200 $\overline{\text{c.m.}}^3$.

III. Washing precipitate $BaCl_2$.

Adjustment 4 min., filling 2 min., washing 18 min., total 24 min. Time apparatus at work, 1 h. 40 min. Amount filtrate 1 liter 250 $\overline{\text{c.m.}}^3$.

IV. Washing $Al_2O_6H_6$.

Adjustment 1 min., filling 1 min., washing 5 min., total 7 min. Time apparatus at work, 1 h. 50 min. Amount wash water 525 $\overline{\text{c.m.}}^3$.

While at work the apparatus requires no attention whatever. It can be adjusted so that the height of the liquid in B will not vary more than a millimeter. By fitting B with a hot water jacket, which can be very easily done, precipitates which easily oxidize can be thoroughly washed and kept at 100° out of contact with the air.

The whole cost of the apparatus need not exceed three or four dollars, and usually all that will have to be bought is the electro-magnet. Other necessary parts will be found in the laboratory.

I am indebted to the kindness of Mr. Ed. H. Ketcham of Dartmouth College for the accompanying drawing.

ART. XL.—*Investigations on Parasulphobenzoic Acid;* by IRA REMSEN.

(Concluded from page 282.)

V. *Attempts to prepare Orthosulphobenzoic Acid.*

The fact that in the crude sulphotoluenic acid, employed in the preparation of parasulphobenzoic acid as above described, two varieties are contained, viz: the ortho- and para-; and, further, the fact that the crude product of the oxidation, in the form of the potassium salts, when fused with potassium hydroxide yielded, as we have seen, salicylic as well as paraoxybenzoic acid, showed plainly that the ortho-acid had not been destroyed by the oxidizing agent. The process was in other cases continued for a long time, and a large excess of the oxidizing mixture employed; and still salicylic acid was obtained, its quantity as compared with that of paraoxybenzoic acid appearing rather to be increased than diminished under these circumstances.

This result was not what might have been anticipated after Fittig had called attention to the fact that ortho-compounds conduct themselves toward oxidizing agents differently from the compounds of the two other series, the former being, as he states, completely destroyed, yielding carbonic acid and water. In the case under consideration two explanations might be given; either the orthosulphotoluenic acid had been converted into orthosulphobenzoic acid; or it had withstood the action of the oxidizing mixture; for orthosulphotoluenic acid itself when fused with potassium hydroxide yields salicylic acid, if the heating be continued long enough, as was shown by Wolkow (loc. cit.).

The solution which yielded the acid barium parasulphobenzoate was evaporated down gradually, and, after it was reduced to a small volume, it was still found to contain a considerable quantity of an easily soluble salt, differing entirely from the difficultly soluble parasulphobenzoate. This was recrystallized a number of times, and finally obtained in the form of microscopic needles; though even after repeated recrystallizations its appearance hardly warranted the conclusion that it was a pure compound. It was, as stated, easily soluble in water; and the difference between its solubility in cold water and that in hot was not very great; so that the crystallization was necessarily brought about by allowing the solution to stand for a length of time over sulphuric acid. The salt thus prepared was analyzed after being thoroughly dried over sulphuric acid.

0·4215 grams of the salt, on being heated to 240°, lost 0·0155 grams H^2O; and then gave 0·2228 grams $BaSO^4$=0·121 Ba.

	Calculated.		Found.
$(C^7H^7SO^3)^2$	342	68·80	
Ba	137	27·58	28·71
H^2O	18	3·62	3·68
	497	100·00	

Though the results of this analysis agree but poorly with the calculated percentages, it nevertheless makes it appear exceedingly probable that this easily soluble substance is nothing but the salt of orthosulphotoluenic acid. Acid barium sulphobenzoate requires much less barium (23·10 per cent) and much more water (3 mol.=9·11 per cent). The fact that more barium was found than is required by the theory would appear to indicate that the salt was rendered impure by the presence of a small amount of a neutral barium salt of sulphobenzoic acid. As the neutral barium salt of parasulphobenzoic acid corresponds in its properties very nearly to the salt here described, this becomes still more probable. Be this as it may, it is evident that the CH^3 group of orthosulphotoluenic acid has not been acted upon by the oxidizing mixture, nor has the acid been destroyed. The same results were obtained when a large excess of sulphuric acid and potassium bichromate were allowed to act upon the mixture of the two sulphotoluenic acids for a long time; so that the statement of Fittig that the ortho-compounds are destroyed by oxidizing agents requires qualification. It seems indeed from this and a subsequent experiment, that the ortho-acid is acted upon with much less energy than the para-acid, if acted upon at all. This case agrees, however, with those mentioned by Fittig in the fact that the toluene-derivative yields no corresponding derivative of benzoic acid.

The destruction of aromatic compounds is by no means confined to those which belong to the ortho-series. The case of salicylic acid mentioned by Fittig is indeed no proof of his general statement, inasmuch as all aromatic oxyacids, as far as I have subjected them to experiment, are destroyed with equal facility. I have, for instance, treated salicylic, oxybenzoic, paraoxybenzoic, protocatechuic and gallic acids with sulphuric acid and potassium bichromate, and in each case exactly the same phenomena were observed. Gentle heating was sufficient to commence the process, which then proceeded violently to the end, accompanied by an evolution of carbonic acid, without the further aid of heat. On now examining the mixture, not a trace of a solid product could in any way be discovered. So that salicylic acid does not differ in this respect from other aromatic acids containing OH; and its conduct toward oxidizing agents is of course no proof that it belongs to the ortho-series. This does not, however, detract from the value of Fittig's exceedingly interesting observation; it merely serves to define it more accurately.

That the ortho-compound in the special case under consideration is acted upon less energetically than the para-compound, was also shown in a rough way by the following experiment. The potassium salts of the two sulphotoluenic acids were separated very nearly by means of crystallization. The pure para-salt was introduced into the oxidizing mixture, and the reaction that ensued carefully observed. The same quantity of ortho-salt, still containing some of the para-salt, was afterward introduced into the same quantity of the oxidizing mixture as was employed in the former case, and the reaction compared with the former one. A striking difference was observed. Whereas the reaction commenced very quickly with the para-salt, and a strong evolution of gas took place; with the ortho-salt it was necessary to apply heat for a longer time before the reaction fairly began, and then the process was markedly more sluggish, a very slow evolution of gas continuing for a long time. By means of approximate quantitative experiments, further, it was shown that the longer heat was applied to the vessel in which the oxidizing process was going on, the smaller was the yield of para-acid; but in no case did I succeed in completely destroying either the ortho- or para-acid. This shows that the para-acid is very susceptible to the action of the oxidizing agent, the oxidation taking place apparently at first in the methyl-group and then extending gradually to the whole molecule; whereas the ortho-acid resists the same influence strongly, and if oxidation takes place at all, it breaks up the compound, yielding the last products of combustion. Whether the same is true of other cases I am unable to say; facts do not speak against it at present, and further experiment can alone decide this point, which certainly possesses more than ordinary interest.

I was thus forced to abandon the hope of obtaining orthosulphobenzoic acid by the oxidation of orthosulphotoluenic acid by means of sulphuric acid and potassium bichromate. It is possible that other oxidizing agents, as for instance potassium hypermanganate may yield more satisfactory results. A preliminary experiment made with this salt in an alkaline solution showed that oxidation took place readily; and I shall soon commence the study of this reaction in detail.

VI. *Oxidation of the Amides of Sulphotoluenic Acid.*

The difficulty with which ortho-compounds are obtained and the importance of studying them in a pure condition, in order to complete our knowledge of their conduct under various conlitions, led me to undertake one more experiment, with the hope of finding a method for the preparation of orthosulphobenzoic acid. The experiment failed to bring about the desired object, but at the same time gave other interesting results, an account of which follows.

According to A. Wolkow (loc. cit.) the amides of the two sulphotoluenic acids can be separated from each other by means of crystallization. This statement offered a prospect of obtaining an ortho-compound in pure condition; and I hence prepared a quantity of the amides from the crude mixture of potassium salts of sulphotoluenic acids. The perfect separation of the two by means of crystallization is an exceedingly tedious operation, whether water or alcohol be employed as the solvent. From water the paramide crystallizes first, and can then easily be purified; from the mother-liquor a mixture of the two amides is deposited. This fuses at 124°, and can only be resolved into its constituents by a very long series of recrystallizations. I succeeded at last in obtaining a small quantity of the ortho-amide in a pure condition, with all the properties as given by Wolkow.

Now as the amide contains the group $SO^2.NH^2$ instead of the sulpho-acid group $SO^2.OH$, it seemed possible that its conduct toward oxidizing agents might differ from that of the sulpho-acids. No attempts had as yet been made to oxidize compounds containing such a complicated group as $SO^2.NH^2$, and I was obliged to gain a certain amount of preliminary knowledge before proceeding to subject the ortho-amide to oxidation. For this purpose I introduced a few grams of parasulphotoluen-amide into a noted amount of the oxidizing mixture (sulphuric acid and potassium bichromate); and heated the whole gently for a short time. Soon the oxidation began, as was shown by a change in color and an evolution of gas. The amide dissolved rapidly, and, soon after it had completely disappeared, a beautifully crystallized product began to make its appearance, and increased constantly in quantity. After cooling, the liquid was filtered off. The solid product remained on the filter, after being washed out with cold water, in a pure white condition. It consisted of beautiful, short, lustrous prisms that bore no resemblance to the original amide. It was found to be easily soluble in alkaline carbonates, carbonic anhydride being evolved, and was reprecipitated from these solutions in crystalline form on the addition of mineral acids. It was almost insoluble in cold water and difficultly soluble in hot water; and when only once crystallized from water, it had the form of flattened prisms, sometimes more than an inch in length. These had a high luster, and while in the solution exhibited a very beautiful fluorescence. It was found to fuse at a very high temperature, but to undergo decomposition before this point was reached. These properties distinguish the new substance from the amide of parasulphotoluenic acid. Its composition was determined by the analysis.

I. 0·8657 grams of the substance, thoroughly dried over sulphuric acid, were oxidized in a silver basin with saltpetre and potassium hydroxide (Liebig's method). There were obtained 1·03 grams $BaSO^4 = 0{\cdot}14146$ grams S.

II. 0·5505 grams of the dried substance were heated with soda lime, and the vapors collected in dilute hydrochloric acid. In this way were obtained 0·262 grams Pt, corresponding to 0·037163 N.

	Calculated.		Found.
$C^7H^7O^4$	155	77·11	
S	32	15·92	16·34
N	14	6·97	6·75
	201	100·00	

These results agree with the formula $C^6H^4\left\{\begin{matrix}SO^2.NH^2\\CO.OH\end{matrix}\right.$, and this formula agrees with the method of formation of the substance. We have:

$$\underbrace{C^6H^4\left\{\begin{matrix}SO^2.OH\\CH^3,\end{matrix}\right.}_{\text{Sulphotoluenic Acid.}}\qquad\underbrace{C^6H^4\left\{\begin{matrix}SO^2.NH^2\\CH^3,\end{matrix}\right.}_{\text{Sulphotoluenamide.}}\qquad\text{and }\underbrace{C^6H^4\left\{\begin{matrix}SO^2NH^2\\CO.OH.\end{matrix}\right.}_{\text{New Acid.}}$$

I have given the acid the name *parasulphaminbenzoic acid*, as indicating the constitution which it undoubtedly possesses.

A compound of a similar constitution, but belonging to another series, is already known, viz: the so-called *sulphobenzamic acid.* This was prepared by Limpricht and Uslar* and by Engelhardt.† The former obtained it by heating sulphobenzamide, $C^6H^4\left\{\begin{matrix}SO^2.NH^2\\CO.NH^2,\end{matrix}\right.$ or ammonium ethylsulphobenzoate with potassium hydroxide; further, by treating the compound $C^7H^6N^2SO^2$ (obtained from sulphobenzamide by treating with phosphorus chloride) with caustic potassa. In both cases ammonia is eliminated. Engelhardt obtained it by means of a complicated reaction, viz: the action of sulphuric anhydride on benzonitrile, other products being formed at the same time. Sulphobenzamic acid being derived thus from ordinary (meta) sulphobenzoic acid retains in all probability the meta-position of the substituting groups; and, although it is not proved by experiment, it is probable that the amide-group is in this case also situated in the sulpho-group and not in the caryl: the name would indicate the contrary, but the analogy this compound shows with parasulphaminbenzoic acid, in h the amide group is undoubtedly situated in the sulpho- ıp, leads me to believe that the name is incorrect; and that name metasulphaminbenzoic acid would be more in accord- with the internal character.

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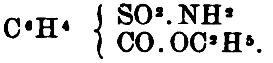
* Annalen der Chemie u. Pharmacie, cvi, 27.

† Jahresberichte, 1858, S. 278.

o the account given above of the method of preparation and properties of parasulphaminbenzoic acid, I need only add a details. For the oxidation the following proportions were d to be most favorable: To 20 grams potassium bichro- are taken 30 grams ordinary sulphuric acid, diluted with times its volume of water; this mixture sufficed for the ation of 7 grams of the pure amide. The amide is intro- d into the mixture after the latter has become cool. At the flask is heated by means of a very small gas-flame. A erately violent action takes place, and in a short time the le is entirely dissolved; now in a few minutes the separa- of the oxidation-product begins. The mass increases con- tly in quantity until the liquid has the form of a thick In about an hour from the beginning of the process the ation is completed. The whole is now allowed to cool n to the ordinary temperature, the product is filtered off washed out with cold water, until the wash-water passes ugh colorless. On the filter is the parasulphaminbenzoic in the form of beautiful crystals, which only require to be stallized from water once in order to be made perfectly . It is easily soluble in alcohol and crystallizes from this ent in smaller crystals than are obtained from water; and do not exhibit the property of fluorescence. It is precip- d in crystals both from a hot and cold alcoholic solution by r.

lphobenzamic acid crystallizes, according to Limpricht and r, in scales like potassium chlorate; according to Engel- t in rhombohedral crystals or in needles, consisting of ag- ates of small rhombohedrons. Heated above the melting t it volatilizes in white vapors. Distilled with phosphorus achloride it yields a number of products among which is chlorbenzoyl chloride. It may be expected that parasul- ainbenzoic acid will under the same treatment yield para- benzoyl chloride—a point which I shall decide by experi- t.

hyl parasulphaminbenzoate,  $C^6H^4\left\{\begin{array}{l}SO^2.NH^2\\ CO.OC^2H^5\end{array}\right.$ This tiful compound is the most characteristic derivative of the It is prepared in the usual manner by conducting dried ochloric acid gas into a solution of the acid in absolute ol, and afterward heating gently on a water bath. Water gives no precipitate in the solution thus obtained. The alcoholic solution must be evaporated down to the consistence of syrup; it then congeals on cooling, and consists of a mass of fine, colorless needles. In water it is less soluble than in alcohol; in cold water much less than in hot. When boiled with water it melts in the liquid before dissolving. From the aqueous solution it separates in the form of long, beautiful needles of a silken luster. These arrange themselves nearly parallel, and may attain the length of several inches. In connection with the melting point, this substance exhibits interesting though perplexing phenomena. In order to be sure that it was absolutely pure I subjected it to recrystallization a number of times; though each time it was obtained in the same form, and possessed in the highest degree the appearance of a chemically pure substance. On endeavoring to determine the melting point, however, I was surprised to find that this varied according to circumstances. When first heated it melted entirely at 110–111°; if it were now removed from the bath and allowed to solidify, it melted immediately after at 94–95°; the longer it was allowed to stand after the first melting, the higher the melting point became, until finally, in about two hours, it again reached 110–111°. Specimens examined at different intervals showed melting points which varied between the limits mentioned; every time that the substance was melted once and then allowed to congeal, and again immediately examined, the melting point was found to be 94·95°. This, taken together with the fact that the substance was pure, is certainly very remarkable. It is possible that this case belongs in the same category with that observed by Zincke* in connection with the two modifications of benzophenone, which is in its turn decidedly inexplicable.

Of the ether a sulphur estimation was made as follows:

0·2849 grams of the substance, dried over sulphuric acid, were oxidized with KOH and NO^3K (Liebig's method), and gave 0·2903 grams. $BaSO^4$=0·03987 S.

	Calculated.		Found.
$C^8H^{11}O^4N$	197	86·03	
S	32	13·97	13·99
	229	100.00	

Ethyl sulphobenzamate also crystallizes according to the description in "splendid, shining needles;" these were determined to be monoclinic prisms. No determination of the melting point appears to have been made. "It dissolves easily in warm alcohol; somewhat less easily in boiling water."

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* Berliner Berichte, IV Jahrgang, 576.

Barium parasulphaminbenzoate, $(C^7H^5SO^4)^2Ba+H^2O$. This salt is prepared by boiling the acid with barium carbonate. It is easily soluble in cold and hot water, and crystallizes in nodular aggregations.

The analysis gave the following results:

0·497 grams salt, dried over sulphuric acid, lost, at 200°, 0·0161 grams H^2O; and gave 0·2094 grams $BaSO^4$=0·12313 grams Ba.

	Calculated.		Found.
$(C^7H^6SO^4N)$	400	72·08	
Ba	137	24·68	24·77
H^2O	18	3·24	3·24
	555	100·00	

The corresponding salt of sulphobenzamic acid crystallizes with $4H^2O$ as "a soft, wavellitic, crystalline mass;" it gives off its water at 110°.

Ammonium parasulphaminbenzoate, $C^6H^4\left\{\begin{matrix}SO^2NH^2\\CO.ONH^4\end{matrix}\right.$, prepared by dissolving the acid in ammonia, is easily soluble in cold and hot water. It easily forms supersaturated solutions, which congeal on being disturbed. It crystallizes in beautiful needles or long laminæ.

The ammonium salt of sulphobenzamic acid crystallizes in laminæ.

The new acid is thus sufficiently well characterized as a chemical individual by these experiments; but as it represents a class of derivatives differing from others in their more complicated nature, it deserves a more thorough examination. It is susceptible to the action of ordinary reagents, and yields compounds with them. It can be boiled with concentrated nitric acid without undergoing change, it being thrown down in crystalline form on the addition of water. It is insoluble in fuming nitric acid at the ordinary temperature; but dissolves when heat is applied. On the addition of water to the solution no precipitate is formed; on evaporating, however, a very easily soluble, colorless, crystalline product was obtained. Fuming sulphuric acid also dissolves it with the aid of heat, and as water gives no precipitate with the solution, it is probable that a sulpho-acid is formed; though in this connection it should be remembered that the isomeric body, sulphobenzamic acid, appears to yield sulphobenzoic acid when treated with sulphuric anhydride (Limpricht and Uslar). The study of these reactions I shall take up in due time. Parasulphaminbenzoic acid having a symmetrical structure, the investigation of its sulpho-acid offers a possibility of throwing light upon the constitution of the tribasic acids of benzene; the conversion of the two sulpho-groups into carboxyl may succeed, though, after the experience of Ascher* with sulphoterephtalic acid, this is hardly probable.

I have already stated that the object of undertaking the oxidation of the amides of sulphotoluenic acids was to open another road with the hope of arriving in the end at a method for the preparation of orthosulphobenzoic acid; I also stated that this object was not attained. Orthosulphotoluenamide still containing some of the paramide, was subjected to oxidation. It was immediately noticed that, as in connection with the sulpho-acids, the action in this case was not as violent as in the case of the para-compound. After heating for some time the product was examined. The parasulphaminbenzoic acid formed was filtered off and the filtrate extracted with ether. The ethereal extract, on being distilled, left behind a residue consisting of orthosulphotoluenamide with a very little parasulphaminbenzoic acid. The latter can be readily removed by redissolving the whole in water, adding a little alkali to the solution, and then again extracting with ether. In this way absolutely pure ortho-amide can be obtained. This was again subjected to oxidation, and, after treating for a length of time, no new product could be discovered in the liquid—a portion of the substance only having been destroyed. Thus we see that the conduct of the ortho-amide is exactly analogous to that of the corresponding sulpho-acid; and this serves to strengthen the conclusion drawn in regard to the conduct of ortho-compounds in general toward oxidizing agents.

The points which have been left unsettled thus far in this investigation will be considered in a second paper on a basis of experiments.

Williams College, Mass., December, 1872.

ART. XLI.—*The Salt deposits of Western Ontario;* by JOHN GIBSON, B.A., Principal of the Almonte High School, Ont.

Extent of the Salt-bearing area.—The superficial area of the Ontario salt deposits is comparatively small. Its northern, northeastern, eastern and southeastern limits seem to be pretty clearly definable by means of the numerous artificial perforations that have of recent years been made in this portion of the Province. From such observations it appears that the whole salt-bearing district may be included within the counties of Huron and Bruce. These portions of Ontario lie along the eastern shore of Lake Huron; and are bounded on the north and east respectively by the counties of Grey and Wellington, and on the south by the county of Lambton. Numerous borings in search of salt have, however, been made in other districts of the Province, but all such attempts have finally proved fruitless. Direct experiment, therefore, forces us to the conclusion that by far the greater volume of salt is to be found under the waters of Lake Huron, and this view is partly corroborated by the fact that at Port Austin, in Michigan, which lies almost due west of the Ontario salt region, there was extracted at the depth of 1,198 feet from the surface a brine marking, according to Dr. Gœssman, 88° salometer, and containing 17·61 per cent of sodium chloride. This latter perforation indicates at the outset sandstones of the Chemung period; but as the depth increases, certainty regarding the exact geological formation attained diminishes in like ratio. However, it may with some degree of plausibility be conjectured that in this boring, as in those in Ontario, the source of the brine is to be found in the Salina formation of the Upper Silurian series of rocks. Yet it would be altogether unsafe to state unhesitatingly that such really is the case, seeing that a very great diminution in the average volume of the strata overlying the base of the Salina formation is unmistakably presented. From the foregoing data it is extremely probable that this ancient geographical depression or salt-basin had an eastern and western extension of at least 85 miles, with probably a much greater stretch from north to south.

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* Annalen der Chemie u. Pharmacie, clxi, 3.

Geological features of this Salt area.—The fundamental rocks of this district belong, with but one or two exceptions, to the *Corniferous limestone formation* of the Middle Devonian system. These Devonian rocks of Ontario are represented by portions of the Oriskany sandstone, Corniferous limestone (including the Onondaga limestone), Hamilton, Portage, and Chemung groups. The following is given as a table showing approximately the geological position of the different formations observed, either as outcrops or by borings in the area in question; and it is given in full in order that it may include all the formations that present themselves in the numerous borings for salt in the vicinity, and that the relative position of any subdivision may at once be recognized:—

I. Middle Devonian,	Corniferous limestone, including the Onondaga limestone. Schoharie grit (not observed in Ontario).
II. Lower Devonian,	Cauda-galli grit (not observed in Ontario). Oriskany sandstone.
III. Upper Silurian,	Lower Helderberg group of Vanuxem, including only the fundamental *Tentaculite limestone.* Onondaga formation, Salina group of Prof. Dana.

IV. Middle Silurian,	Guelph and Galt formations.	
	Niagara formation.	Anticosti group.
	Clinton "	
	Medina "	

Of the subdivisions of the Middle Devonian system one is only found within the area under review. This is the *Corniferous limestone* formation, which forms by far the greater portion of the underlying surface rock. The Lower Devonian is not apparently represented in these counties, although numerous fragments of the Oriskany sandstone are scattered here and there as angular and lately detached erratics. The rocks of the Lower Helderberg group are represented only by the Tentaculite limestone or so-called water-lime beds. These latter are met with in two distinct exposures, each of which presents similar lithological characters. The Onondaga salt group or Salina formation is found to extend under the whole district, so far as can be ascertained by borings, forming the foundation rock, so to speak, of the water-lime group, and when this is absent, immediately underlying the Corniferous and Onondaga limestones. The Guelph formation—the uppermost subdivision of the Middle Silurian series, is only observed by artificial borings at the depth of about 1,150 feet from the surface of the ground, and underlying the most recent deposits of rock salt. Of the presence of the Niagara, Clinton, and Medina formations, we have but very doubtful evidence; and it is only by means of specimens of rock brought up by the sand pump, during the operation of boring, that we arrive at the probability of their existence within the average depth of 1,200 feet from the surface.

Living in the center of this salt region, I have been enabled to make frequent visits to the various salt-wells during the operation of drilling, to collect the detailed "logs" or records of each boring, and to arrive at some very important generalizations regarding the distribution and volumes of the underlying Upper Silurian and Devonian series of rocks in this locality. In order to see at a glance the character and geological sequence of the strata, it has been deemed advisable to furnish the records in full, thereby bringing to light many facts replete with novel interest and geological phenomena

The following is a list, proceeding from north to south, of the principal wells sunk and still in practical operation:—

1. Kincardine well.
2. The Ainleyville well.
3. Goderich Company's well.
4. The Dominion well.
5. Hawley's well, Goderich.
6. Clinton well.
7. Stapleton well.
8. Coleman and Gowinlock's well, Seaforth.

I. *The Ainleyville well.*

	Feet.
(1.) Sand, gravel, with boulders of gneiss and granite,	16
(2.) Gray and blue limestones; the uppermost 100 feet probably belonging to the Corniferous formation, whilst the remaining 266 feet are magnesian, and evidently belong to the Salina,	366
(3.) Layers of chert interstratified with bands of limestone,	180
(4.) Beds of compact *steatite* or *soapstone*, alternating with layers of magnesian limestone, with disseminated crystals of bitter spar,	353
(5.) Gray magnesian limestone,	97
(6.) Magnesian limestone containing traces of brine,	168
(7.) Dark-brown porous sandstones,	64
Total depth,	1244

At this depth the well was abandoned. Saline waters were met with at the depth of 1,012 feet, and were probably derived from the saliferous stratum lying further south. The position of the boring seems to mark the northeastern margin of this ancient salt lake, since the geological horizon of the salt was passed without the least evidence of its occurrence. The 97 feet of gray magnesian limestone seemingly belong to the base of the Onondaga formation, below which no brines of any economical value have yet been found. At the depth of about 1,200 feet a small water-course was met with, in which were observed traces of petroleum and bubbles of vicious gas. The brine extracted from this well was obtained only at intervals for fifteen feet, having been first observed at the depth of 1,006 feet from the surface. Specimens at no time marked over 30° salometer; they gave a specific gravity of 1·054, and consequently contained only 7·71 per cent of pure salt.*

2. *Kincardine well.*

	Feet.
(1.) Passed through the Corniferous limestone, the Tentaculite limestone, to the base of the Salina formation (the records imperfect),	883
(2.) Pure rock salt,	27
(3.) Magnesian limestone,	30
(4.) Pure rock salt,	17
Total depth,	957

Here the occurrence of a second layer of salt, separated from the first by 30 feet of limestone, leads to important considerations regarding the probability of its extension *at all parts* under the first. Indeed, to restrict the presence of this second deposit to very narrow limits within a salt-area comparatively wide, would be not only to controvert the laws regulating the distribution of sedimentary beds, but also to render obsolete and void all the known theories connected with salt deposits generally. On consideration of the oscillations of level necessary for the deposition of a vast bed of salt, with the evidence of a second saliferous layer in one perforation only, the writer maintains that, within the limits of this salt-district of Ontario, a second saliferous deposit exists universally, at slightly variable distances below the first, except, probably, in the neighborhood of the margin of this ancient geographical depression.

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* For a table giving the strength of brines from zero to saturation, see Prof. Alexander *Winchell's Report* on the Geology of Michigan, 1861.

3. *Goderich Company's well.*

		Feet.
(1.)	Sand, gravel, and boulders,	30
(2.)	Soft arenaceous limestone, with a layer of calcspar,	266
(3.)	Hard gray sandstone, with slight traces of salt and petroleum,	78
(4.)	Blue magnesian limestones,	330
(5.)	Magnesian limestone, holding numerous crystals of calcspar,	110
(6.)	Porous limestone, gypsum, and alternating bands of red marl and salt,	163
(7.)	Rock salt,	45
	Total depth,	1022

In the above well boring, commenced on the 17th Nov., 1865, and at the expiration of exactly 102 days, the salt rock was reached at about 1,000 feet from the surface. From this depth there was obtained, by pumping, a saturated brine, from which large quantities of salt continue to be manufactured. The salt-bearing stratum lies immediately at the base of the Onondaga formation, and is at once recognized by the presence of saliferous and gypsiferous magnesian marls lying as a general rule above the salt bed.

4. *The Dominion well.*

		Feet.
(1.)	White and blue clays, holding boulders of Huronian and Laurentian origin,	97
(2.)	Water-lime beds (Tentaculite limestone),	48
(3.)	Soft arenaceous limestones,	362
(4.)	Hard magnesian limestones,	331
(5.)	Very hard dolomitic limestones, holding crystals of melanterite (sulphate of iron),	87
(6.)	Limestone and shale in alternate layers,	120
(7.)	Compact limestone and gypsiferous shales,	47
(8.)	Rock salt,	21
	Total depth,	1113

After boring through 21 feet of pure rock salt, the underlying limestone was reached, and at this depth the boring ceased. The *Corniferous* limestone is here absent; the first strata reached having the character of the so-called *water-lime* beds. As shown in the record, we have for the entire thickness of the Onondaga formation at this particular locality 968 feet, of which the upper 807 are chiefly magnesian limestones, with occasional cherty layers, the underlying 161 feet being represented by gypsiferous and saliferous shales, including the mass of rock salt at the base. The brine pumped up constantly marks 87° salometer, and has a specific gravity of 1·175 at the temperature 62° F.

5. *Hawley's well, Goderich.*

The record of this well was essentially the same as that of the "Dominion," until the salt deposit was reached at the depth of 967 feet, after which the drilling was carried (1) through 12 feet of impure salt and shale, and (2) through 17 feet of pure rock salt.

6. *The Clinton well.*

		Feet.
(1.)	Clay, gravel, sand, and boulders,	70
(2.)	Gray cherty non-magnesian limestones (Corniferous),	108
(3.)	Water-lime beds (Tentaculite limestone),	24
(4.)	Hard magnesian limestones, with intercalated beds of chert,	283
(5.)	Hard arenaceous limestones, with beds of shale and gypsum,	470
(6.)	Coarse limestones and gypsiferous shales, with a mud-vein 3 inches in thickness,	147
(7.)	Very porous limestone, containing salt,	14
(8.)	Rock salt,	20
	Total depth,	1136

Nothing of particular interest marked the process of boring in the above well. The presence of the *Corniferous* formation was at once detected by the borers, constituting 108 feet of the surface rock. The underlying 938 feet constitute the Salina formation (Onondaga), which here does not attain such a thickness as at Goderich, the upper portion probably having been removed by erosion prior to the deposition of the Corniferous beds. Hydrated calcium sulphate or gypsum was met with about the center of the Salina formation, occurring in compact snow-white masses along with crystals of selenite (a lamellar form of the same).

7. *Stapleton well.*

		Feet.
(1.)	Boring commenced 40 feet below the general level of the country,	40
(2.)	Sand, gravel, and boulder clay,	67
(3.)	Light-gray limestone, intercalated with numerous mud-veins,	413

(4.)	Chert or siliceous stone, containing variable quantities of magnesia,	204
(5.)	Stratified limestone, uppermost 4 feet tolerably pure, the rest containing variable quantities of silica and magnesia. From 780 to 810 feet from the surface the rock approaches true dolomite,	176
(6.)	Shales intercalated with thin beds of clay,	80
(7.)	Crystalline brown limestone,	25
(8.)	Brown and white magnesian limestone, alternating with beds of shale and gypsum,	115
(9.)	Blue clay intercalated with gypsum,	45
(10.)	Cellular limestone, shale, and gypsum,	26
(11.)	Rock salt,	15
(12.)	Shale, gypsum, and rock salt,	14
	Total depth,	1220

This well is situated on lot 39, first concession, in the township of Hullet, 13½ miles to the southeast of Goderich, and on the line of the Buffalo and Lake Huron railway. It was sunk in 1867 by Mr. Ransford, the proprietor, and still continues to yield brines of great strength and purity, the proportion of earthy chlorides being comparatively small. The uppermost 200 feet of rock were found to belong to the Corniferous formation, which is here again observed covering the summit of the Salina group. Water-fissures were met with at the respective depths of 161 and 466 feet; but there was no evidence of the present existence of water in them. Crystals of calcspar occurred at the depth of 400 feet, and at 780 feet crystals of selenite $(CaSO^4+2H^2O)$. At 952 feet a bed of compact gypsum several feet in thickness was encountered, and at 1,005 feet a layer of pure alabaster.

Before reaching the salt horizon a sudden transition from fresh water to strong brine was observed, at about 1,100 feet from the surface. Such an occurrence may be explained by the hypothesis, that an impermeable argillaceous shale completely excluded the fresh water of the upper layers of limestone from the lower saliferous rocks.

Finally, it may be mentioned that the prevalence of vast quantities of gypsum and salt in a mixed state naturally suggests the utility of a shaft, by which not only could pure rock salt be obtained, but also the combined gypsum and salt for agricultural purposes.

8. *Coleman and Gowinlock's well, Seaforth.*

		Feet.
(1.)	Gravel, sand, and clay,	25
(2.)	Stratified dark-gray limestone,	400
(3.)	Stratified magnesian limestone, followed by a very hard layer of chert,	200

(4.) Crystalline siliceous limestone, containing magnesia,	110
(5.) Blue clay, shale, and limestone,	250
(6.) Gypsum, shale, and salt,	50
(7.) Rock salt,	100
Total depth,	1135

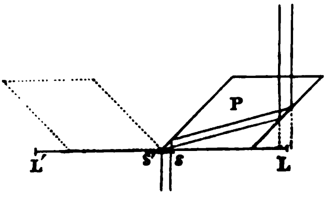
The drilling done in this well was unprecedented in the annals of this system of mining, both for speed and absence of mishaps. Actual boring commenced on the 10th of March, 1870, and the salt-bearing stratum was reached on the eve of the 22d of the same month. After passing through 100 feet of pure rock salt, without the least evidence of change, the boring was abandoned. The great success attending this boring led to the sinking of two other wells, viz:

Sparling and Merchant's, in the immediate vicinity; both, however, giving records similar to the above.

Truly in no other portion of the American continent has there been discovered a deposit of salt so magnificently great. The supply is practically illimitable, and may favorably compare with the production of the salt mines of Droitwich, in Central England, or with that of the solid salt-hills of Cordova. In a future paper I shall take occasion to describe the different *systems of manufacture* at present pursued in Western Ontario.

ART. XLII.—*Comparison of the Spectra of the Limb and of the Center of the Sun, made at the Sheffield Scientific School;* by CHAS. S. HASTINGS.

A COMPARISON of the spectrum of the edge of the sun with that of its center is of great theoretical interest; but any comparison other than by direct juxtaposition must be very unsatisfactory, and the more so as the differences are less. In order to obtain spectra of two different portions of the sun side by side, where the slightest variations may be detected, I have constructed a small prism with four polished sides, its bases being parallelograms. This is so placed that one face rests upon the slit plate of the telespectroscope, and has its acute edge perpendicular to the slit at its middle point. The instrument may then be directed so that the image of the sun falls with its center on the uncovered portion of the slit, while the light which forms the edge of the sun, falling perpendicularly upon the first surface of the prism, suffers two interior total reflections and a displacement depending upon the form of the prism. A glance at the



figure, in which $s s'$ is the slit, L L′ the diameter of the sun's image, and P the prism, shews that no light from the covered part of the slit will reach the collimating lens except that which has been reflected from the two sides of the prism. The relation of the acute angle (v) and the distance between the reflecting sides (t) to the focal length of the great telescope (F) and the width of the spectrum (a) is given by the formula,

$$2t \sin v = \mathrm{F} \tan 16' - a.$$

The sides of the prism not fixed by the equation admit of considerable latitude, but should be made to approach the lower limit in order that the planes of the direct and transmitted images may be as little separated as possible. Of course t and v should be so proportioned that the reflections may be total.

The instruments with which the following observations have been made are those belonging to the observatory of the Sheffield Scientific School, consisting of an equatorial telescope of 9 in. aperture, and 118 in. focal length, by Clark, and a spectroscope of Young's form by the same maker. The spectroscope has a dispersive power of 12 prisms of 60°. In most of these observations an eye-piece of high power has been adapted to it, which gives a separation of the D lines equal to 64 minutes nearly. In the small prism placed before the slit, a is equal to .04 in., a quarter of the length of the slit.

When the instrument is properly directed and in adjustment we see a very narrow black line dividing the spectrum longitudinally into two parts of widely different intensity; the fainter, belonging to the limb of the sun, is marked on its edge by the bright chromosphere lines. Upon comparing these two spectra, certain differences are recognized besides that of intensity, by far the most marked of which are exhibited by the lines b_1 and b_2, which become sharper and less hazy near the limb. The lines b_3 possesses the same characteristic, but to a less degree; C and F also become sharper in the same region. Excepting these and the D lines it requires very close examination to detect any variation. There is, however, a line in the red at 768·1 of Kirchoff's scale which is strongly marked near the center of the sun's disk, but disappears entirely, to my power at least, within 16″ to 20″ from the limb. Two other lines below F, at 1828·6 and 1830·9 of the same scale, exhibit nearly complementary phenomena, i. e., they are strongly marked near the edge, but much fainter at the center. These latter lines also become greatly strengthened over the penumbræ of spots. The line 768·1 is not thus affected. These are all the differences which I have invariably seen in repeated examinations since the 17th of February.

Others have, however, been suspected. Certain lines, which are strengthened in a region of spots like those above mentioned, appear to be strengthened also near the edge, but do not undergo so marked a change. It is obvious that the differences should be most pronounced in the clearest sky, and such is the case. The closest examination has extended only from B to a short distance above F, as the plate glass of which the small prism is made has a decided yellow tint and absorbs the blue rays strongly.

Since the light from the border of the sun undergoes a general absorption, which reduces its intensity to much less than one-fourth that at the center, according to Secchi's measurements, and yet the spectroscopic character is changed so slightly, it is impossible for me to escape the conviction that the seat of the selective absorption, which produces the Fraunhofer lines, is below the envelope which exerts the general absorption. But the phenomena of the faculæ prove not only that this envelope rests upon the photosphere, but also that it is very thin. The origin of the Fraunhofer lines, then, must be in the photosphere itself, which is in accordance with Lockyer's views.

Any effects which the chromosphere might produce, we would anticipate finding most evident in the lines of those gases which are readily detected there. A reference to the observations shows at once a compliance with this anticipation in the lines of hydrogen, magnesium and sodium. The line 768·1 is not less strikingly in concordance, if it be regarded as 768·?* (the ? indicates doubt as to the tenths of the scale and * absence of a corresponding black line) of Young's Catalogue of Chromosphere lines. The lines 1828·6 and 1830·9, with others of the same class, probably have their origin in the medium which exerts the general absorption, and thus are allied to our telluric lines. It also seems probable that the chromosphere is too transparent to reverse many of its lines. That this is the case in the helium lines is tolerably certain.

In the apparatus described, two similar prisms were also placed over the slit in a symmetrical position. The spectra of two opposite edges of the sun were thus brought together, and the change in refrangibility due to the sun's rotation was very clearly shown.

New Haven, April 3d, 1873.

ART. XLIII.—*Contributions from the Physical Laboratory of Harvard College.*—No. IV. *Induced currents and derived circuits;* by JOHN TROWBRIDGE.

THE expression for the intensity of an induced current, deduced by Neumann and Sir William Thomson, is as follows: $i=\frac{1}{k}\frac{dU}{dt}$, in which k is a coefficient depending upon the resistance of the complete wire in the secondary circuit, and U is a certain "force function" which depends solely upon the form and position of the wire at any instant, and on the magnetism of the influencing body. The expression, in general language, is as follows:

"When a current is induced in a closed wire by a magnet in relative motion, the intensity of the current produced is proportional to the actual rate of variation of the "force function" by the differential coefficients of which the mutual action between the magnet and the wire would be represented if the intensity of the current in the wire were unity."

This investigation was undertaken to ascertain if the laws of derived circuits apply to the currents of induction, which are represented by equations of which the above is a type. A reflecting galvanometer of large resistance was included in the secondary circuit, and connected by copper wires of very small resistance with the coil in which the secondary currents were produced: the resistance of these wires was infinitessimal in comparison with the resistance of the galvanometer. The galvanometer was then shunted. The first two columns of the following table show that, with an inappreciable resistance outside of the galvanometer coils, the shunts made no difference in the deflection of the galvanometer needle when the shunts were not less than three ohms. Below this the current divided. The resistance of the galvanometer was 5880 ohms, and the last numbers in the second and third columns show that an equal impulse was transmitted through both the shunt and the galvanometer, for no reason can be assigned why it should take

Exterior Resistances, in ohms.	Shunts, in ohms.	Deflections.	Exterior Resistances, in ohms.	Deflections.
0	3	210	10	210
"	4	210	20	210
"	5	210	30	210
"	6	210	40	210
"	5880	210	100	190

one course in preference to the other. Two galvanometers, therefore, of the same resistance, one forming the shunt to the other, will give the same deflection, which is equal to that given by the undivided circuit.

Resistances were then introduced in the circuit exterior to the galvanometer coils, and a shunt of 588 ohms was used.

The fifth column shows that no effect was produced by the shunt until the exterior resistance was appreciable in comparison with that of the galvanometer.

The following table exhibits the effect of resistances which were appreciable in comparison with the galvanometer resistance. The same shunt of 588 ohms was used. The second column is calculated on the assumption that

$i = \frac{dU}{dt}$, (where k' is a coefficient), is equivalent to $i_{1} = \frac{E}{R}$, and

$'R$

that the laws of Kirchoff hold. The third column is obtained from the experimental data. The fourth and fifth columns are also calculated on the assumption that $i = \frac{E}{R}$ = tangent of the deflection. Columns second and third are expressed in arbitrary scale divisions.

Exterior Resistances, in ohms.	Calculated value, of *i*.	Experimental value, of *i*.	Ratio of intensities.	Ratio of Tangents.
1500	1242	1375		
2000	1033	1055	1·06	1·03
2600	954	990	1·06	1·04
3000	767	825	1·06	1·04
3500	673	770	1·11	1·05
4000	613	660	1·05	1·03
4500	558	649	1·05	1·07
5000	514	550	1·04	1·03

It will be seen by comparison that, with large resistances exterior to the galvanometer resistance and appreciable in connection with it, the laws of the division of currents practically hold, and as the exterior resistance approaches that of the galvanometer, the coincidences with the laws is more marked.

From the above it appears that, under certain conditions, an induced current does not divide according to the laws of divided circuits, but approximates to these laws when there is a resistance exterior to the galvanometer which is appreciable in comparison with that of the galvanometer.

No. V.—*On a method of measuring induced currents;* by F. H. Bigelow.

If a Wheatstone's bridge be formed, in which the secondary coil of the inductorium is the resistance R' to be measured, the relation between the resistances will be expressed by the proportion $R_1 : R_2 = R_3 : R_4$ where R_3 and R_4 are in a fixed ratio. By passing a current from an independent battery through the primary coil, the strength of the current in R_2 will be increased or diminished on the breaking or making of the inducing current. Therefore, to preserve equilibrium in the bridge, R_1 must be changed, and we shall have $R_1 \pm C : R_2 \pm x = R_3 : R_4$, in which x is a resistance equivalent to the effect of the induced current. Let $R_1 \pm C$ be found by trial such that the addition or subtraction of the induced current will produce equilibrium in the bridge; that is, will be such as to bring the deflection of the galvanometer to the zero of the scale. We shall then have the strength of the induced currents expressed as resistances. One advantage of this method is this, that the readings are always reduced to the same point, the zero of the scale. This method also has a wider range than that of merely taking the swing of the galvanometer needle. For currents which would throw the spot of light in a reflecting galvanometer off the scale, can be readily kept on the scale in this method by merely altering the ratio of R_3 to R_4 in the Wheatstone's bridge. Since the shunting of induced currents is accompanied with difficulties, as will be seen by the paper of Mr. Trowbridge accompanying this, this method is especially advantageous. When the bridge was set up so that the smallest variation in the resistance of the branch containing the inductorium gave the greatest variation in the current going through the galvanometer, namely, when $\frac{dS_0}{dR_3} = 0$, S_0 being the current through the galvanometer, and the resulting value of R_3 being $R_3 = \sqrt{\frac{GR_1(2B+R_1)}{G+2R_1}}$, in which G is the resistance of the galvanometer, B that of the circuit exterior to the Wheatstone's bridge, it was found that the induced currents could be measured to one hundred-thousandth of an ohm.

The following table contains a comparison of the induced currents produced by making and breaking the circuit. The first two columns contain the variation in ohms of the variable resistance of the bridge; the third and fourth columns give the strength of the induced currents on making and breaking expressed in ohms.

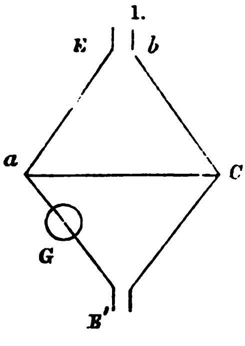
Change in the resistance on breaking.	Change in the resistance on making.	Strength of Induced Current on breaking.	Strength of Induced Current on making.
650	600	·00325	·00300
700	680	·00350	·00340
720	720	·00360	·00360
720	750	·00360	·00375
700	700	·00350	·00350
850	850	·00425	·00425

Care should be taken to send the induced currents to be compared in the same direction by means of a pole changer. It will be seen from the above table that the equality of the currents on making and breaking can readily be proved by this method.

No. VI.—*On methods of determining the resistance of a battery, deduced from Poggendorff's mode of measuring Electromotive Forces;* by N. D. C. Hodges.

In the process of obtaining the ratio between the electromotive forces of two cells, the expression $\frac{E'}{E}=\frac{B+R_0}{R_0}$ is obtained; in which $E>E'$, R_0 is the resistance necessary to bring the needle of the galvanometer G to zero, and B is the resistance of the battery to be measured. By introducing a new resistance R′ in the branch *ab*, we shall obtain the expression $\frac{E}{E'}=\frac{B+R'+R_0+R_1}{R_0+R_1}$, in which R_1 is the new value of R_0; hence $\frac{R_1}{R'+R_1}=\frac{R_0}{R+R_0}$ and $B=\frac{R_0R'}{R_1}$. If the ratio $\frac{E}{E'}$ becomes $\frac{m}{n}$ we shall have

$$\frac{m}{n}=\frac{B+R_0}{R_0} \text{ or } B=R_0\left\{\frac{m}{n}-1\right\}$$

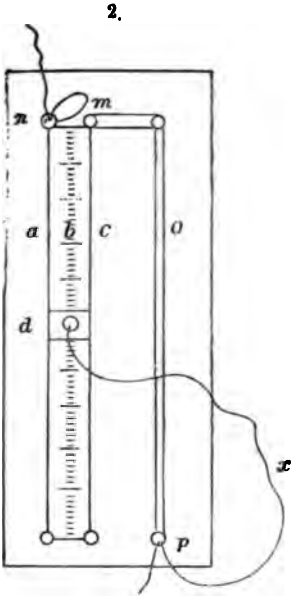


This expression was given by Mr. Mance in the Phil. Mag., vol. xli, p. 318, 1871, and is seen to follow directly from Poggendorff's method of measuring electromotive forces. The following are the results obtained from the expression $B=\frac{R_0R'}{R_1}$, in measuring the resistance of two Grove cells. One Grove cell was opposed to the two whose combined resistance was measured.

R_0	R′	R_1	B
·357	1	1·028	·347
"	2	2·057	·347
"	5	5·144	·347
"	7	7·140	·350

By Mr. Mance's method, with the same value of R_0, the value obtained was $B = \cdot 357$. The above numbers are expressed in ohms and fractions of an ohm. My method has the advantage that many determinations of the battery resistance can be made with the same arrangement of cells. In Mr. Mance's method but one determination can be made without altering the arrangement. I have found the following form of Rheocord useful in these determinations.

2.



In fig. 2 a and c are two German silver wires, with a slider at d which has a binding screw; between the wires is a mirror b with a scale upon it. or at its side, in order to avoid parallax in reading. $O\rho$ is a German silver wire or rod of small resistance.

As the slider is now arranged, the current coming in at n will divide at d, pursuing the two paths $d\chi\rho$ and $dmo\rho$. By placing a box of resistance coils at χ, we can make the combined resistance of $d\chi\rho$ and $dmo\rho$ differ as little as we please from the standard resistance $o\rho$. It is thus possible, by making a large variation in χ, to measure small resistances a little larger than the small resistance $o\rho$; thus employing the correct principle of working from the greater to the less.

By disconnecting the resistance $o\rho$ at m and ρ and joining mn, it will be readily seen that the combined resistance of the circuit $dnmd$ is one-half that of nd alone. It is often desirable to reduce the resistance in this manner.

The changes in the combined resistances can of course be readily calculated from the expression $\frac{\chi\eta}{\chi+\eta} = R$, where χ and η represent the two branches of the circuit. I have often found it advantageous in measuring large resistances to shunt them, so to speak, in the Wheatstone's balance, thus forming a combined resistance, one branch of which includes the large resistance and the other a carefully measured resistance much smaller than the resistance to be measured; the balance can in this manner be kept in its most sensitive arrangement, and a series of measurements can be made of the large resistance by varying the carefully measured portion of the combined resistance.

SCIENTIFIC INTELLIGENCE.

I. CHEMISTRY AND PHYSICS.

1. *On the Action of Charcoal on Organic Nitrogen.*—STANFORD has made a series of experiments to test the opinion of Stenhouse, which has since become current in science, that nitrogenous matters in contact with charcoal become oxidized to nitrous or nitric acids. Three mixtures with charcoal were made; of meat, of urine, and of excreta. Two months afterward, and every succeeding month for six months, the mixture was tested; no appreciable loss of nitrogen could be detected, nor was any nitric acid formed. Subsequently, three typical charcoals, from wood, from seaweed, and from bone, were mixed with finely chopped lean beef, equal weights of the charcoal and the meat being taken. The mixtures were placed in loosely stoppered bottles, and tested occasionally for a period of 21 months. The most marked action of the charcoal was to dry the mixtures, they losing 40 per cent of water in three months. No trace of nitric or nitrous acid could be detected throughout the experiments. Moreover, no difference was observed between bone and wood charcoal, though the one contains nine and the other 92 per cent of carbon; which could hardly be the case did carbon act as an oxidizing agent. A slight loss of nitrogen occurred, apparently as ammonia. The author concludes: (1) Charcoal, when thus applied, acts simply as a drier; (2) it does not favor oxidation and the production of nitrates; (3) after a long time and if artificially dried, the mixture may lose a little nitrogen as ammonia; (4) this loss for all practical purposes, is inconsiderable.—*J. Chem. Soc.*, II, xi, 14, Jan., 1873. G. F. B.

2. *On the Specific Heat of Occluded Hydrogen.*—Graham concluded, from his experiments on palladium when saturated by hydrogen, that these two substances formed an alloy. If this be so, then, since the specific heats of alloys approach closely the mean specific heat of their constituents, it ought to be possible to obtain the specific heat of the hydrogen when in this condition. ROBERTS and WRIGHT have undertaken the solution of this problem. Pure palladium wire, also wire of palladium and 20 per cent of gold, were electrolytically charged with hydrogen, then washed with water and alcohol, heated to 100° C., and then plunged into a calorimeter, every precaution being taken to guard against error. In some of the experiments the palladium was charged to saturation, in others only partially charged, and in still others it was saturated and a part of the charge was expelled before making the determination. After the measurement, the hydrogen was expelled by heating the wire in the vacuum of a Sprengel pump, and was measured. The specific heat of palladium alone was found to be between 15° and 100°, 0·05964, increasing to 0·06022 after four times charging. That of the

occluded hydrogen varied from 4·06, where 5·4 grams of hydrogen were contained in 1000 of palladium, to 9·10, where 1·45 grams were thus occluded. The results obtained with the palladium-gold wire, corroborate those given by the palladium alone. The authors attribute the variations in the results to the irregular distribution of the hydrogen through the palladium. Moreover, since the specific heat of bodies when liquid is higher than when solid or gaseous, it might be supposed, inasmuch as the highest specific heats are found where the charge of hydrogen is lowest, that the hydrogen first absorbed is in a quasi-liquid condition, and that taken up subsequently quasi-gaseous. If so, then as the passage from the one to the other is uniform, it would indicate a continuity between the quasi-liquid and quasi-gaseous states of matter. Or, on the other hand, Graham's supposition is untenable, and the substance is not an alloy, but approaches more the ordinary solution of a gas in a liquid. Moreover, since the specific heat does not vary directly with the quantity of hydrogen, it cannot be a mixture of a definite palladium hydride with an excess of palladium. It follows, therefore, "that each several charge must be regarded as giving rise to a distinct compound, and, therefore, that palladium and hydrogen, like hydrogen chloride and water, are capable of entering into combinations . . . in proportions which are *not* expressible by simple formulæ."—*J. Chem. Soc.*, II, xi, 112, Feb., 1873. G. F. B.

3. *On the Purple of Cassius.*—The purple precipitate obtained when a dilute solution of gold is treated with a mixture of stannic and stannous chlorides, is generally regarded as a compound, probably of stannous and aurous oxides. Debray has investigated this question, and is disposed to regard the purple of Cassius as a stannic (or metastannic) lake, colored by finely divided gold. If a mixture of solutions of stannic chloride and sodium acetate be boiled, stannic oxide is precipitated. If now a little gold chloride solution be added to the hot liquid, and then potassium oxalate, the gold is at once reduced, a small quantity is deposited upon the glass, but the larger quantity upon the tin, producing the ordinary purple of Cassius. Moreover, a similar substance may be obtained in a similar way with alumina. To a solution of gold chloride saturated with sodium acetate, gelatinous alumina is added and the mixture is heated; the addition of a little potassium oxalate reduces the gold and produces the purple lake. Both these lakes, suspended in water, and agitated for many hours with mercury, retain their color perfectly. The solubility of the purple of Cassius in ammonia is explained by the fact that stannic oxide is also so soluble when moist; the same influences injuring this solubility in the one case as in the other. This solution of the purple of Cassius when made is turbid by reflected light and slowly deposits metallic gold; a fact impossible to explain on the old theory. As to the purple insoluble in ammonia, obtained by the assayers in dissolving in nitric acid, silver containing a little tin, and gold, Debray shows that if the solution be

effected hot, the stannic oxide and the lake resulting are both insoluble; while if the heat used be very moderate, a purple of Cassius is obtained in this way entirely soluble in ammonia.—*Moniteur Scientifique*, III, ii, No. 372, 1007, Dec., 1872. G. F. B.

4. *On the Determination of Free Oxygen in Solution.*—**Schützenberger** and **Gérardin** have made some experiments upon the determination of free oxygen by titrition with a graduated solution of the new sodium hydro- (or more properly, hypo-) sulphite, discovered by the former, which, as is well known, has a strong attraction for oxygen. In presence of free oxygen, it becomes sulphite at once:

$$HNaSO_2+O=HNaSO_3.$$

A half hour before the determination, a flask of 60 to 100 c.c. capacity is filled three-quarters full with water, a spiral of leaf zinc is placed in it, and to it is added 10 c.c. of a solution of hydrosodium sulphite of 20° B. It is then filled with water and closely stoppered. By occasional agitation the reduction is effected in 20 to 25 minutes. It is graduated by placing in a test glass 20 c.c. of a solution of copper (copper sulphate in ammonia, 10 c.c. of which, in passing from cupric to cuprous oxide, lose 1 c.c. of oxygen) covering this with a layer of oil. By the side of this is placed a bottle containing a liter of the solution to be tested, also covered with oil, and previously colored blue with Coupier's blue (which is decolorized at once by the hyposulphite, while the sulphite does not affect it) used as an indicator. Into a burette of 50 to 60 c.c. capacity divided into tenths c.c. the hyposulphite solution is placed. Its titer is first ascertained by noting the amount necessary to decolorize the copper solution. Then at once and from the same burette, the solution is added to the liquid to be examined until it is decolorized. During these operations, the opening of the burette is kept below the level of the oil. By a simple calculation, the amount of dissolved oxygen is easily ascertained.—*Moniteur Scientifique*, III, iii, No. 373, January, 1873. G. F. B.

5. *On Coloring Matters from the Aromatic Azodiamines.*—**Hofmann**, having studied three years ago Magdala red, a color produced by the action of naphtytamine upon azodinaphtyldiamine, has lately, in connection with **Geyger**, undertaken to study the results of this reaction in other cases. They find that both aniline and toluidine, heated with azodiphenyldiamine in a sealed tube to 160° for four or five hours, produce a blue coloring matter, which they call *azodiphenyl blue.* The reaction is:

$$C_{12}H_{11}N_3+C_6H_7N=C_{18}H_{15}N_3+NH_3.$$

This blue is a strong base, dark brown in color, insoluble in water, soluble in alcohol and ether, and colored blue by acids. Heated in alcoholic solution with hydrochloric acid and zinc turnings, it yields a leucobase. The aniline-violet obtained by Girard, DeLaire and Chapoteant, by the oxidation of aniline, has also the empirical composition $C_{18}H_{15}N_3$; but the authors were not able to satisfy themselves that they are identical. They also examined *safranine*, an orange coloring matter, formed by the oxidation of aniline. It is a toluidine derivative, $C_{21}H_{20}N_4$, formed by the union of three molecules of the monamine, with the substitution of one atom of nitrogen for three of hydrogen, and the elimination of four atoms of hydrogen.—*Ber. Berl. Chem. Ges.*, v, 472, 526. 1872. G. F. B.

6. *Sensibility of the Eye in determining differences in intensity of different colors.*—Helmholtz gives to the Academy of Berlin the following analysis of the work of W. Dobrowolsky, of St. Petersburg, on the sensibility of the eye in determining differences in intensity of different colors. If two colors of the same tint and of different intensities are placed side by side, the eye appreciates their difference in intensity as long as it exceeds a certain fraction of the intensity of the light observed, as Fechner first made known. This fraction for white light is about $\frac{1}{100}$, and in certain exceptionally favorable cases it equals $\frac{1}{150}$ or $\frac{1}{180}$. The author has made researches on different regions of the solar spectrum, with the assistance of two other observers, and gives the following figures for the values of the denominator of the fraction.

First observer.

Red,	A,	14·	Blue,	*E* to *b*,	67·3
"	B,	19·76	"	F	131·6
"	C,	25·16	Indigo,	G	268
Orange,	C to D,	33·16	Violet,	G to H	268
Yellow,	D,	45·77	Violet,	H	67
Green,	D to E,	58·77			

		Second observer.	*Third observer.*
	B	15·9	11·7
	D	40·86	27·5
Between	G and H	205·5	205·5

The intensity of the violet was not sufficient to give the maximum sensibility. We see from these results that the sensibility of the eye in its appreciation of differences in the intensity of colors, increases as we go from the red to the violet. A. M. M.

7. *Galvanic reduction of Iron under the influence of a powerful electro-magnetic solenoid.*—Jacobi, of St. Petersburg, covered the interior walls of two glass vessels with cylinders of sheet-iron, and placed in these vessels two similar rods of wax, coated first with a thin electro deposit of copper, and then with plumbago. The vessels were then filled with a solution of sp. gr. 1.27, containing 135 parts of ferrous sulphate, and 123 parts of magnesium sulphate, rendered neutral by the addition of magnesium carbonate. One of these vessels was surrounded by a tube of sheet iron, in which was coiled a helix of insulated copper wire. A reducing current from one Smee cell was now passed through the solution in the two vessels, while another current from four Bunsen cells went through the magnetizing helix. At the end of twenty-eight days the wax rods were examined, with the following results: An

equal weight of iron was deposited on each rod; but, while the iron on the rod not exposed to the heliacal current was smooth and fair, the iron on the other rod was principally on its upper and lower portions in the form of tufts, having a crystalline structure, and resembling somewhat the appearance presented by a bar magnet after its introduction into iron filings.

Jacobi found that both deposits were very feebly magnetic, and further experiments showed that iron deposited by electolysis receives a remarkably high charge of temporary magnetism, and has very feeble coercive force; he therefore recommends such iron for the construction of electro-magnetic cores.—*Ann. de Ch. et de Phys.*, Feb., 1873. A. M. M.

8. *Ozone and Antozone. Their History and Nature. When, Where, Why, How is Ozone observed in the Atmosphere?* by CORNELIUS B. FOX, M.D., Edin., Member of the Royal College of Physicians, London, etc., etc. 330 pp., 8vo. 1873. Illustrated. London: J. & A. Churchill.— A very satisfactory account of the numerous investigations into the nature of ozone and antozone, into their natural occurrence, and the methods of identifying them, as well as of the observations that have been made upon them and the conclusions based on these observations. It is a critical account also of these several matters, not indeed, exhaustive, but very fair and just. On the whole, an excellent book, very suggestive, very compendious, well arranged, and as a guide, aid, and stimulus to new investigations, invaluable. The whole matter of the natural occurrence and offices of ozone needs renewed, comprehensive and systematic research. The conflicting conclusions and observations hitherto made reveal a most unsatisfactory state of knowledge upon a topic which is of high and universal interest. The first preliminary to the needed new investigations is just such a laborious collation of all existing work as Dr. Fox has given us. S. W. J.

9. *Elements of Natural Philosophy;* by Professors Sir W. THOMSON and P. G. TAIT. Part I. 279 pp. 8vo. 1873. Clarendon Press Series: Macmillan & Co.—This work consists in great part of the large type, or non-mathematical portion of the first volume of the *Treatise on Natural Philosophy* by the same writers. That volume was published in 1867, and was designed to be followed by three others. It has been for some time out of print, and we are glad to see an announcement of a second edition of it. Many persons, however, will prefer this smaller work, as the omitted mathematical portions of the *Treatise* are not always elementary. The mathematical methods employed in the *Elements* are, almost without exception, limited to those of the most elementary geometry, algebra, and trigonometry. It is designed for use in "schools and in Junior classes in Universities." The subjects treated of in this volume are Kinematics, Dynamical Laws and Principles, Experience, Measures and Instruments, Statics of a particle, and Statics of solids and fluids.

II. GEOLOGY AND NATURAL HISTORY.

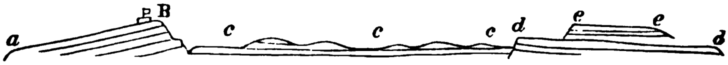
1. *Notes on the Island of Curaçao;* by W. M. GABB. (Letter to J. D. DANA, dated Curaçao, Jan. 20, 1873.)—Curaçao is one of a series of barrier islands lying off the coast of Venezuela, at a distance of about 30 to 40 miles from the main land. It is a long barren strip, nearly 40 miles long, with a trend to the southeast. Its surface is in the main flat, and but little elevated above the sea level. The bay of Curaçao, around the mouth of which the town is built, is a long intricate body of water, divided into several arms, and, entering from the south side, almost bisects the island.

The geological structure is extremely simple. There is but one rock formation and that is what I have termed the "Coast limestone" in my memoir on Santo Domingo. (Trans. Amer. Phil. Soc.) It is lithologically identical, being an amorphous carbonate of lime, containing occasional fossil shells and corals, all of living species. Unlike the rock in Santo Domingo, for which I suggested the term *Antillite*, it shows no soft earthy or chalky character, so far as I have been able to examine the few existing excavations. But I suspect that deep quarrying would develop this "chalk," since it occurs in most if not all of the other islands, at a depth beyond the reach of atmospheric influences.

Back (north) of the town is the bed of a dry water course; a few shallow pits full of brackish water show that here as in the Bahamas, Bermudas and elsewhere, the fresh surface water exists, floating on top of the sea water, which percolates through all the cracks and cavities of the rock. (See Nelson's memoirs in the Trans. and Quart. Jour. Geol. Soc.)

The stratification of the rock is usually horizontal or nearly so; but the few hills in the vicinity of the city show that the island owes its existence to the presence of a well marked anticlinal axis. The direction of the upheaval is coincident with the trend of the island, and the dip on the back side is seaward, that is to say, the hills on the north side of the island show a northern dip, while those on the south side have the rocks dipping about 20° southward.

In the immediate vicinity of the city, a series of hills run to the westward, their northern or inland faces being quite precipitous, while the southern sides invariably have the slope of the rock strata. Some of these reach a height of 100 feet. One, immediately back of the city, I estimated to be about 130 to 150 feet high. The central line of the island is nearly flat, divided into a slightly rolling surface, but with an elevation so much less than the hills, that it can only be accounted for by the existence of a fault. North of this again, bordering the north coast, at least in part, is



a, a, sea level; B, citadel back of the town, about 130 feet high; *c, c, c,* internal rolling plain; *d, d,* table land bordering the north coast; *e, e,* high hill (100 ft.) off the line of section.

a table land, abrupt inland and sloping slightly seaward, thus giving as a cross section of the island, in the vicinity of the city, a structure like the preceding.

The soil of the island is scanty except in the little village in the interior (*c*, *c*, of section), where it has accumulated through the aid of rains. Here fruit trees are cultivated with great care. Elsewhere the rocks are not infrequently entirely bare. There is no vegetable mould and the earth is red, the same characteristic "red earth" or clay as that found in Jamaica, Santo Domingo, the Bahamas, &c. The climate and vegetation are very similar to the rainless parts of Santo Domingo. Showers are very rare, springs, properly speaking, do not exist and the water supply is derived principally from carefully constructed cisterns; the deficiency being made up by poor water obtained from the shallow wells. The plants are of the same species, at least in part, as those found between Santiago and Monte Cristo, Santo Domingo. Two or three species of stunted acacias, or Cereus, 10 to 12 feet high, and one or two species of Opuntias are the most striking. Another noticeable feature is the large number of individuals of two species of land shells, a Pupa and a Cyclostoma, which literally swarm over the trunks of the trees and lie in little heaps around their roots. Besides these, lizards a foot and a half long, by thousands, make up the greater part of the native fauna. Very few birds are found, and I have not seen a single snake.

I have learned but little of the neighboring islands. On Little Curaçao there is a deposit of guano, now being mined on a large scale. Buenos Ayres shows a series of peaks, several hundred feet high, but I am told that they are composed of the same rock as that of Curaçao. Here also, there are guano deposits, but the guano is said to be poor in phosphoric acid. The whole region, including the adjoining parts of the main land, is dry and arid, and, as the result of careful inquiries, I am led to believe that the large peninsula adjoining the bay of Maracaibo is also of the same horizontal Post-pliocene rock.

2. *Spergen Hill fossils identified among specimens from Idaho;* by F. B. MEEK. (Communicated.)—A remarkable group of very small fossils has long been known to occur at a locality called Spergen Hill, near Bloomington, Indiana, at about the horizon of the St. Louis limestone of the Lower Carboniferous series. The most singular peculiarity of these fossils is, that, although showing every evidence of being adult forms, most of them are of *very* diminutive sizes, and look like the merest miniature representations of known larger species. They belong, for the most part, to well known genera of *Corals*, *Blastoïdea*, *Brachiopoda*, *Lamellibranchiata*, *Gasteropoda*, *Cephalopoda*, &c., and are found in a very beautiful state of preservation, crowded together in immense numbers.

A few of these little fossils have been found at the same horizon in Illinois, Iowa, and Missouri, scattered through the rocks, along with larger species; but at no other locality have so many of the species been found crowded together, in such vast members, as at Spergen Hill. Nor am I aware that any of the species have been hitherto identified from any locality west of Missouri and Iowa.

In studying last summer's collections of Dr. Hayden's party, I was much surprised to see several of these little Spergen Hill fossils, in a dark gray crumbling mass of limestone, found by Prof. Bradley, of Dr. Hayden's corps, on the divide between Ross Fork and Lincoln valley, Idaho. This mass was only about seven inches in length, by three to four inches in breadth and thickness. On breaking it to pieces, I found it to contain hundreds of little fossils, belonging to about seventeen species of the same genera found at Spergen Hill; and a careful comparison also shows that about one half of these seem to be, so far as can be seen, *specifically* undistinguishable from Spergen Hill forms.

Prof. Bradley has since broken up the fragments more carefully and found a few additional species. The following is a list of these fossils. Those preceded by an asterisk are believed to be in all respects identical with species found at Spergen Hill; while the others are at least more or less closely allied representatives of forms occurring at that locality:

Cyathaxonia, (like a Spergen Hill species.)
*_Pentremites conoideus_ Hall.
Pentremites, (like _P. Koninckianus_ Hall.)
Rhynchonella mutata Hall?.
*_Rhynchonella macra_ Hall.
*_Retzia Verneuiliana_ Hall.
Athyris, (representing _A. Shumardi_ H.)
Spirifer, (like _S. Norwoodi_ H)
*_Productus biserialis_ Hall.
*_Nucula Shumardiana_ Hall.
*_Conocardium Meekianum_ Hall.
*_Cypricardella plicata_ Hall.
*_Cypricardina Indianensis_, (=_Cypricardia_ H.)
*_Straparollus_ (_Euomphalus_) _Spergenensis_ Hall.
Pleurotomaria, (undetermined sp.)
Bellerophon, (like _B. sublævis_ H.)
Naticopsis, (like _N. Carleyi_, = _Natica Carleyi_ H.)
Platyceras, (undt. sp.)
Cythere, (like _C. carbonaria_ H.)
Phillipsia, (like a Spergen Hill sp.)

The occurrence of so many identical and representative species of such peculiar diminutive fossils, crowded together in the same way, at these two widely separated localities, is, to me, a very curious and interesting fact.

Washington, D. C., Feb., 1873.

3. *On the Probable Existence of Microscopic Diamonds, with Zircons and Topaz, in the Sands of Hydraulic Washings in California;* by Prof. B. SILLIMAN.[*]—The occurrence of diamonds of some size in the gold fields of California is by no means uncommon, and was noticed by me in a communication to the California Academy of Science in 1867, when specimens of this gem, from at least five different localities, were exhibited. I then suggested that a more attentive examination of the heavy sands left in the sluices of hydraulic washings would in all probability detect diamonds, mingled with other rare species not commonly believed to occur in these sands.

Mr. George A. Treadwell, of San Francisco, has lately sent me a small package of these sands, collected by him from the sluices of the "Spring Valley Gravel Mining Claim," Cherokee, Butte county, California. A microscopic examination shows these sands to abound in beautiful colorless zircons (hyacinths), of the form well-known in the hyacinths of Expailly (France), associated with crystals of topaz, quartz in fragments, rounded grains of chromic and titanic iron, and a few small, almost globular, masses of very high refracting power which appeared to be diamonds. To determine this chemically, a portion of the sands was treated with acid for the removal of any carbonates which might be present. There was no effervescence from this treatment. The same sample was then digested in strong sulphuric acid of a high temperature to destroy any particles of organic matter which might be present, washed out in pure water without contact with organic matter, dried and ignited in a vessel of platinum out of contact with air. This sample of the sands thus freed from anything which could afford carbonic acid, but the diamond, was then ignited in a platinum nacelle (boat) in a tube of hard glass, and in a current of pure dry oxygen gas, which, for precaution, was passed over soda lime, and then, after passing the ignited assay, was delivered through a solution of baryta water. The transparency of this delicate test was soon disturbed, and by continuing the experiment for about an hour, a notable quantity of baryta carbonate was obtained. This experiment seems to prove that diamond powder was present in small quantity. When I am provided with a larger quantity of these sands, I propose to determine the amount of diamond dust quantitatively.

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[*] A paper read before the American Institute of Mining Engineers, Boston, Feb. 19th, 1873.

It will be remembered that Prof. Wöhler, some years since, found diamonds by a similar method in the platinum sands from Oregon, associated with the rare species *Laurite*—sulphide of osmium and ruthenium. His paper will be found in the American Supplement to the Chemical News for November, 1869, p. 317.

The black grains, which contribute fully one-half the bulk of the Butte county sands, are about equally chromic iron, which the magnet removes, and titanic iron, which is unaffected by the magnet. The chromic iron was so proved by the blowpipe, and no magnetite could be detected. No metallic grains of any of the platinum or iridosmium metals, or gold, could be found.

In his letter to me, accompanying the sample sent, Mr. Treadwell says: "I have examined much of the sand under the microscope, and think there are a few fragments of broken diamonds. These sands were taken from the tailings after passing through a long flume paved with stone. You know what sharp, hard pounding the gravel gets, mixed with boulders, in a hydraulic flume. No doubt, some diamonds are ground, or rather broken, by hard knocks to powder."—*Engineering and Mining Journal*, Feb. 25, 1873.

4. *Prof. Henry Wurtz on Metamorphism as a consequence of the transformation of motion into heat.*—Mr. Wurtz, in a letter to J. D. Dana, dated Hoboken, N. J., April 5th, mentions the fact that he presented the view that the heat of metamorphism was

due to motion in the altered strata, in a memoir read before the Buffalo meeting of the American Association, August 1st, 1866, entitled "Gold Genesis." The memoir was published in the American Journal of Mining, January 25th, 1868, under the heading "Gold-Genetic Metamorphism, with some views on Vein-Genesis," and in it occurs, besides other allusions to the subject, the following paragraph:

"There is one related point to which I shall devote a few words; as due weight may not have been attached thereto by geologists, even supposing that it has occurred to any in precisely the same light. This is, that the tremendous dynamic agencies, whose effects of upheaval, subsidence, disruption and displacement, we find so widely manifest, while doubtless themselves engendered of the pent-up heat-energy of the interior, must have given birth to, or have been in part transmuted into, heat-motion. Hence I deduce conclusions of great moment, but one or two of which can now be dwelt upon. It follows, for instance, that in our theoretical views of metamorphism, we are by no means of necessity limited, for our essential chemical excitant, merely to that portion of the hypothecated residual cosmical heat which might be supposed to have been retained by the emerging oceanic floor. Neither elevation nor subsidence (both necessarily accompanied by enormous compression) could occur without rise of temperature; though the degree of this rise would, of course, vary very much in various parts of the mass. The era of possible metamorphic changes dependent upon the percolation of superheated waters is thus indefinitely prolonged, *even to the present time:* and explanation, both in mode and measure, is thus presented for our thermal springs and many like phenomena." * * *

The foot-note indicated by the star at the close of the last paragraph begins thus: "In passing by for the moment this important subject, I will ask whether the general rise of heat represented as found on descent into European mines, may not possibly admit of a similar explanation," meaning of course heat, or *molecular* motion, derived from *molar* motion, which latter I presume means 'work.'"

In this memoir, Prof. Wurtz stated the probability, if he did not prove the necessity, of the existence of gold in solution still in our present ocean, and predicted its probable future discovery, as lately verified by Sonstadt.

5. *Pitchblende and Tellurium-Gold Ore in Colorado*—Professor N. P. Hill, writing from Blackhawk, Colorado, to one of the editors of this Journal, says: "A discovery was made some time since, about a mile from Central City (Colorado), of *pitchblende*. It is now found in large quantities, several tons of ore containing fifty per cent of uranium oxide having been shipped to England. The ore has commanded a price of $1 per pound thus far."

The same correspondent says: "The Red Cloud Mine has produced considerable quantities of an ore of tellurium rich in gold and silver, the samples which I have seen containing also lead." Samples of both these ores have been sent us by Mr. Hill, and when they arrive we shall hope to determine the species to which the tellurium ore belongs. B. S.

6. *Contributions to a Fauna Canadensis, being an account of the animals dredged in Lake Ontario in* 1872; by H. ALLEYNE NICHOLSON. (From the Canadian Journal.)—A preliminary report of these dredgings was published in the Annals and Magazine of Natural History, vol. x, p. 270, October, 1872. The present paper includes most of the matter of the previous one, with a revised list of the species and descriptions of some that are regarded as new.

The dredgings were all in shallow water as compared with those made by Mr. S. I. Smith, in Lake Superior, in 1871.* The greater part of the species were obtained in Toronto Bay, where the depth was from one to three fathoms. Some dredgings were also made in the open lake, where the water was from 8 to 40 or 50 fathoms deep. But in most of the deeper dredgings very few animals were found. The list of animals obtained includes 43 species, of which 21 are shells, and 6 fishes and reptiles. The minute species are omitted. The shells are all inhabitants of shallow water, and most of them are species that are widely distributed in the fresh waters of the northern United States and Canada. *Valvata tricarinata* was the only species found living at depths as great as 8 fathoms; all the others were from less than 4 or 5 fathoms. Three species of leeches are described and figured as new. One of these, *Clepsine patelliformis*, appears to be perfectly identical with *C. elegans*, described by me in this Journal, vol. iii, p. 132, Feb., 1872. The color differs slightly from the variety originally described, but the color-variety that he describes is not uncommon at New Haven. Prof. Nicholson states that this species carries the young attached to the ventral surface by means of their posterior sucker, and thinks that this is a remarkable habit. He says, "This extraordinary habit of carrying the young has been noticed by Prof. Verrill, in another species of *Clepsine;* but I am not aware that attention has otherwise been drawn to it." In the paper by me, to which he refers (this Journal, Feb., 1872), I gave this habit as a *generic* character (as many other writers had done before me), saying (p. 127), "The young adhere in a group to the posterior part of the lower surface of the body of the parent, by means of the posterior sucker, and before quitting the parent, usually present the essential characters and often nearly the pattern of color of the adult, though paler." And in describing the species, the attached young of four species were mentioned, and more or less fully described.†

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* See this Journal, vol. ii, pp. 373, 448, Nov., Dec., 1871; also, Preliminary Report on the Dredging in Lake Superior, by S. I. Smith, in the Report of the Chief of Engineers to the Secretary of War, vol. ii, 1871.

† I might have added that the eggs before hatching are also retained in the concavity beneath the body, in a cluster, and thus *incubated* till they hatch, the

It is strange that Dr. Nicholson should have overlooked my statements in regard to this point, except in the case of one species, especially as I had the pleasure of sending him my paper, before his first one was written; and it is equally strange that the author of a text book of Zoölogy should not have been aware that this habit has been well-known for at least fifty years, and has been described by nearly all writers upon leeches. This genus of leeches is one of the most common in Europe as well as in America, and is represented by ten or more species in England, where their habits are also well known. Johnston, in his Catalogue of British Non-parasitical Worms, p. 50, 1865, even gives this habit as a peculiarity of the *family*. One would suppose that Dr. Nicholson might have had abundant opportunities, while living in England, to have observed this habit among the English species. His *Clepsine sub-modesta* does not appear to differ from young specimens of one of the common color-varieties of my *C. modesta*. The differences in form mentioned depend merely upon the degree of activity of the animal. His *Nephelis vermiformis* is evidently young, and the description and figure are insufficient for its identification. No characters are given sufficient to separate it from the young of the common *N. lateralis* (Say), or my *N. fervida* from L. Superior. In color it agrees best with the latter.

His *Sœnuris Canadensis*, if correctly described, does not belong to the genus *Sœnuris*, for in the latter the setæ are never *forked*. This character would throw it into another family, even, —near *Lumbriculus*. The same remark applies also to the next species, indicated as "*Sœnuris* sp." His next one, "*Lumbriculus* sp." is described as having "four rows of straight spine-like setæ, arranged in pairs." This would therefore not belong to the same family with *Lumbriculus*, in which the setæ are forked and in fan-shaped fascicles; it probably belongs to *Lumbricus*, or some of the closely related genera.

The only crustacean that the author attempts to name specifically is *Pontoporeia affinis* Lindström, which Mr. S. I. Smith found abundantly in Lake Superior in 1871, and identified *by direct comparison with type specimens, from the Swedish lakes*, sent by Dr. Lovèn (see this Journal, vol. ii, Dec., 1871, and Preliminary Report on Dredging in Lake Superior, Oct., 1871). The crustacean obtained by Dr. Nicholson may, perhaps, be this species, but he does not seem to be quite sure of it. Concerning the other species of Amphipods referred more or less doubtfully to *Gammarus* and *Crangonyx*, it is impossible to say anything, as no characters are given sufficient to indicate the genera, and the members of this group are notoriously difficult to determine without the most careful study. The *Mysis relicta* Lovèn,

leech generally remaining stationary in one place during this period, with that portion of the body that covers the eggs arched, and moving constantly with an undulatory motion, evidently intended to force a constant current of water over the eggs.

found in abundance by Dr. Stimpson and others in L. Michigan,* and by Mr. Smith in L. Superior, and first identified by Mr. Smith, with the species from the Swedish Lakes, by comparison with the specimens sent by Lovèn, was not found by Dr. Nicholson. And although he mentions the occurrence of the *Mysis* and *Pontoporeia* in Lakes Superior and Michigan, and alludes to the great importance of this interesting discovery, he does not mention, in his former paper, nor in the present one, *by whom* these discoveries were made. He in no case gives any credit to Mr. Smith, by whom these species were first recognized as American, and who also fully appreciated the importance of the fact of their identity with the Swedish species. Nor does he mention Mr. Smith's name in connection with the dredgings in Lake Superior, although his researches were of far greater importance, and on a much larger scale than those of Professor Nicholson, extending as they did to the greatest depths of Lake Superior (169 fathoms) and crossing the lake in various directions. Moreover, the two papers giving the results of Mr. Smith's researches had been sent to him, before he undertook his dredging operations, by Mr. Smith, at his request, so that he could not have been ignorant of them.

Some suitable acknowledgement of the labors of Mr. Smith in dredging so extensively, and so promptly publishing the results of his researches, was in justice demanded of Mr. Nicholson, who has evidently derived some benefit from them. Not to mention his name at all in this connection was a kind of discourtesy which, fortunately, is rare among American naturalists of repute.

A. E. V.

7. *Tieghem on the Cotyledon of Gramineæ, etc.*—The old controversy between Richard and Mirbel, early in the century, respecting the structure of the embryo in grasses, was never settled. Reduced to modern morphological terms, the views maintained amount to three. According to the first, the *scutellum* or outermost piece of this complex embryo is the cotyledon, the lobule opposite (which is not always present) represents the second leaf, the *pileolus*, or conical tunic which envelops the parts of the plumule that develop into foliage, is the third leaf, so that the earliest green leaf is the fourth of the series. Under the second view, the scutellum is also the cotyledon, but the opposing

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* A brief account of the first dredging expedition (1870) in L. Michigan, has been published in the Trans. of the Wisconsin Academy of Science, etc., 1872, p. 98, by Dr. P. R. Hoy, who was one of the party and the original discoverer of the *Mysis* and other deep-water species found in the stomach of white-fish (*Coregonus*). His paper has been reprinted in the Annals and Magazine of Nat. Hist., April, 1873, p. 319. Another exploration was carried on in 1871, but the collections of both these expeditions were burned in the Chicago fire. A set of the Crustacea from the former was, however, given to Mr. S. I. Smith, just before the fire, by Dr. Stimpson, with his MSS. names attached. These names were given only provisionally, and without much examination, and should not have been published; but as they have appeared in Dr. Hoy's paper and its reprint, it may be well for me to state here, on the authority of Mr. Smith, who has compared the original specimens, that *Gammarus brevistilus* Stimp. MSS. is *Pontoporeia affinis*, and *Mysis diluvianus* Stimp. MSS. is the same as *Mysis relicta* Lovèn.

lobule a mere appendage of it, the rest as in the first view. According to the third view, the scutellum, with its lobule where that distinctly exists, is an unusual expansion of the axis (caulicle, tigelle, or radicle) below the cotyledon, the pileolus is the cotyledon enrolled and sheathing, and the first green leaf is the second of the proper series. Van Tieghem, in Ann. Sci. Nat., sér. v, 15, has taken up the investigation anatomically, as is his wont, following the clue of the vascular threads (the libero-vascular bundles). He determines that the scutellum is the main body of the cotyledon (it receives vascular bundles, one or more, from the axis as does a cotyledon or leaf, but as any mere enlargement or lateral expansion of axis should not); that the *lobule* (which gets no vascular element) is a mere appendage of the former; and that the pileolus belongs to the cotyledon in a different sense, and is the analogue of the ligule or stipule, and derives its vascular elements, in the form of two bundles, from the scutellum. There are homologous cases in the monocotyledons, as in Canna. But *Gramineæ* and *Cyperaceæ* are remarkable for a greater specialization of parts, and for a profound separation between the part which answers to the lamina of the leaf, the *scutellum*, and the part which represents the "superior sheath," ligule, or stipule, which in this instance takes an extraordinary and independent development. A. G.

8. "*Infelix Lolium.*"—*Darnel*, it would appear, has got a bad name without deserving it. It has long been counted as the exception in its order, "the only authentic instance of unwholesome qualities in the order of grasses." The authenticity of the exception has recently been tested in Scotland, by Mr. A. S. Wilson, who reported the result of his trials to the January meeting of the Edinburgh Botanical Society. As Lindley, in his Medical and Economical Botany, says: "Grains narcotic and acrid, producing fatal consequences when mixed with flour," Mr. Wilson began by taking small quantities of the meal of Darnel, raised by himself, rising from two grains (8 kernels) to fifty grains, then eating a mess of pottage made of a hundred grains, and finally eating cakes made in great part of the husks or bran of Darnel. No symptoms of any kind were experienced. A. G.

9. *Saccardo on certain small Bodies in the Fovilla of Pollen* (Nuovo Giornale Botan. Ital., Dec. 10, 1872.—The author states that botanists are agreed that the minute grains in the contents of pollen consist of starch granules, oil-globules, sugar, and nitrogenized compounds but, so far as he is aware, no observer has yet noticed among them certain minute bodies of well-marked and constant shapes. He detected, in June last, very small oscillating bodies which make up the bulk of the fovilla, and to these he gives the name *Somatia.* The form of the somatia is invariable in the same species of plants, and in plants of the same genus the forms appear to be nearly identical. The plants most carefully studied were *Cucurbita Pepo*, *Eschscholtzia crocata*, *Onagraceæ*, *Portulaca grandiflora*, *Althæa rosea*, whose *somatia* are figured as fusiform, discoid, &c. To observe these small bodies to

the best advantage the author advises that a drop of distilled water should be placed on a few grains of the pollen on a slide, and then the cover should be pressed down so as to crush them. The somatia are seen under a magnifying power of 800 to 1000 diameters to have an oscillating motion which may be referred to the "Brownian movement." Treated with a solution of iodine, the color of these somatia becomes blue; but this tint is marked only in the central portion, while the outer part remains clear. The author does not venture to give any theory in regard to the office of the somatia. G. L. G.

10. *A Contribution to the History of the Fresh-water Algæ of North America*, by HORATIO C. WOOD, Jr., M.D., etc. Washington, January, 1873; published by the Smithsonian Institution.—It is now fully twenty years since the Smithsonian Institution performed an appropriate and most acceptable service by publishing the *Nereis Boreali-Americana* of the lamented Prof. Harvey, thus enabling our students to study the marine *Algæ* of our coasts.' It proved to be one of the most popular of the Smithsonian Contributions to Knowledge. The institution has now enabled our students, and all who are curious in microscopic life, to enter upon the more difficult but not less interesting investigation of the Fresh-water Algæ, by bringing out Prof. Wood's important contribution. The systematic part of this goodly volume consists of 239 pages, of imperial quarto size, in which all the United States species known to the author (exclusive of *Diatomaceæ*) are arranged and described; they are illustrated by twenty-one colored lithographic plates, which appear to be excellent. A supplement contains six species, which are described in Prof. Harvey's Nereis, which Prof. Wood did not consult in season to include in their proper places; and in the preface a fine list of fresh-water species collected by Mr. Olney in Rhode Island and named a long time ago by Prof. Harvey, is reprinted from Mr. Olney's *Algæ Rhodiaceæ*. Any student of these interesting forms may thus infer how much remains to be known of them, and all should unite in thanking Prof. Wood in thus opening the way to their investigation; also for the elaborate bibliography appended to the volume. This fills thirteen pages in double columns, and is an almost exhaustive enumeration of the works and scattered papers which relate to this group of plants. A. G.

11. *Botanical Necrology*, 1872–3.—Our record begins and closes with the names of eminent botanists who have been taken from our own home circle.—The following brief biographical notice of the Rev. Dr. Curtis is taken from the Council's Report to the American Academy of Arts and Sciences (Proceedings, vol. viii, p. 471):—

MOSES ASHLEY CURTIS was born in Stockbridge, Massachusetts, on the 11th of May, 1808. His father was the Rev. Jared Curtis, of Stockbridge, afterward for many years chaplain of the State Prison at Charlestown. His mother was a daughter of General Moses Ashley. He was fitted for college chiefly under his father's tuition, and was graduated at Williams in the class of 1827. Three years afterward he went to Wilmington, North Carolina, as a tutor in the family of Governor Dudley, while at the same time he studied divinity. There he resided until the year 1841, with the exception of a year and a half passed with his father in Charlestown. In the autumn of 1834, he married Miss De Rosset, of Wilmington, who survives him. He took holy orders at Richmond, Virginia, in the summer of 1835; became rector of the Protestant Episcopal Church at Hillsborough, North Carolina, in 1841, and fulfilled the duties of this station for the remainder of his life, with the exception of ten years, from 1847 to 1857, during which he had the pastoral charge of a parish at Society Hill, South Carolina. The degree of Doctor of Divinity was conferred on him by the University of North Carolina, at Chapel Hill. His health for a few years past was sensibly impaired; but he was able to perform his professional duties, and, in a measure, to prosecute his scientific studies, until the 10th of April last, when he died suddenly, probably of heart disease.

Dr. Curtis's attention must have early been attracted to botany, and his predilection fixed by his residence at Wilmington, one of the richest and most remarkable botanical stations in the United States. For it was in the year 1834, after only three years' residence there, that he communicated to the Boston Society of Natural History his first botanical work, namely, his "Enumeration of Plants growing spontaneously around Wilmington, North Carolina, with remarks on some New and Obscure Species." This was printed in the first volume and second number of that Society's Journal; but the original impression having been mainly destroyed by fire, important additions and emendations were made in the subsequent reprint. The author's powers of observation and aptitude for research are well shown in this publication, and it is one of the first of the kind in this country in which the names are accented. In his note upon the structure of Dionæa, or Venus's Fly-trap,—a plant found only in the district around Wilmington, —Dr. Curtis corrected the account of the mode of its wonderful action which had prevailed since the time of Linnæus, and confirmed the statement and inferences of the first scientific describer, Ellis, namely, that this plant not only captures insects, but consumes them, enveloping them in a mucilaginous fluid which appears to act as a solvent. Extending his botanical observations to the western borders of his adopted State, Dr. Curtis was among the first to retrace the steps and rediscover the plants found and published by the elder Michaux, in the higher Alleghany mountains. But for the last twenty-five years, his scientific studies were mainly given to mycology, in which he became a proficient and the highest American authority. His papers upon Fungi, some of which are large, and all are important, were mainly published by the American Philosophical Society, and by the Linnean Society of London. Several of them are the joint productions of Dr. Curtis and of the able English mycologist Mr. Berkeley.

His other published writings mainly are "A Commentary on the Natural History of Dr. Hawks's 'History of North Carolina.'"—a good specimen of his appreciation of exact research and of sharpness of wit wholly free from acerbity; two papers in "Silliman's Journal" on "New and Rare Plants of the Carolinas;" and the botanical portion of the "Geological and Natural History Survey of North Carolina," in two parts;—the first, a popular account of the trees and shrubs, issued in 1860; the other, a catalogue of all the plants of the State, in 1867. This includes the lower Cryptogamia, especially the Fungi, of which he enumerates almost 2,400 species, while the Phænogamous plants are less than 1,900. All our associate's work was marked by ability and conscientious-
ness. With a just appreciation both of the needs of the science
nd of what he could best do under the circumstances, when he
ad exhausted the limited field in Phænogamous Botany within
is reach, he entered upon the inexhaustible ground of Mycology,
hich had been neglected in this country since the time of
hweinitz. In this difficult department he investigated and pub-
hed a large number of new species, as well as determined the
l ones, and amassed an ample collection, the preservation of
ich is most important, comprising as it does the specimens,
wings, and original notes which are to authenticate his work.
his unremitting and well-directed labors, filling the intervals
an honored and faithful professional life, he has richly earned
gratitude of the present and ensuing generations of botanists.
ral years ago he prepared drawings of the edible Fungi of the
ntry, with a view to making them better known in an accessi-
and popular publication; but he was unable to find a pub-
r. He was much impressed with their importance as a source
ood. During the hardships of the Rebellion, he turned his
wledge of them to useful account for his family and neighbor-
; and he declared that he could have supported a regiment
excellent and delicious food which was wasting in the fields
oods around him.

on Francis Spring, Professor of Physiology in the Univer-
f Liége, Belgium, died, January 17, 1872, in the 59th year
age. He was the author of the classical monograph of
dium and *Selaginella*, which was published thirty years
the memoirs of the Royal Academy of Sciences at Brus-
We are not aware that he did any later botanical work.

Von Mohl, Professor of Botany in the University of
en. The following notice is reprinted from the Proceed-
the American Academy for June, 1872, being a part of the
's report:

von Mohl, the acknowledged chief of the vegetable anato-
this generation, died on the first day of April last. He
at Stuttgard, April 8, 1805, the youngest of four brothers
became men of mark in political and scientific life; Julius
ralist and Hugo the botanist being the most distin-
The latter was educated at the Stuttgard Gymnasium

and Tübingen University, where he studied medicine as well as natural history and physics. His first publication, while a student, in the year 1827, was his Essay on the Structure and Coiling of Tendrils and Twiners, written in response to a prize-question offered by the Tübingen Medical Faculty. In it he divined the real nature of the movements which coiling stems and tendrils execute, as has recently been clearly made out. In the following year appeared his inaugural dissertation on the Pores of the Cellular Tissue of Plants, in which his later views and discoveries, respecting the structure, growth, and component parts of cells, as subsequently developed, are already foreshadowed. About this time his choice was made for a scientific rather than a medical career; and he went to Munich to prosecute more advantageously his favorite studies. Here the late Von Martius and Zuccarini were his botanical masters, and Agassiz, Karl Schimper, Braun, and Engelmann his fellow-students. Here he made those researches upon the anatomy of ferns, cycads, and especially of palms,—the latter a most important contribution to Martius's great work upon palms, the former also contributed to another work by Martius,—which first displayed his remarkable talents for histological investigation, to which his subsequent scientific life was mainly devoted. His merits were promptly recognized by a call to the Imperial Botanic Garden of St. Petersburg, as assistant to its director, Dr. Fischer, and to the chair of Physiology in the Academy of Berne. He accepted the latter in 1832, and occupied it until 1835. Then upon the death of Schübler, he returned to Tübingen, accepted the professorship of Botany in its High School, in which chair and in that of Tübingen University the rest of his life was passed. Invitations to more prominent and lucrative positions, as, for example, to the Botanical chair at Berlin University, when vacated by the death of the veteran Link, were unhesitatingly declined. Although he published numerous (about ninety) special papers or articles, most of them important and timely, and some of great pith and moment, he resolutely declined to bring out any general work. His *Mikrographie* (1846) and his "Principles of the Anatomy and Physiology of the Vegetable Cell" are his only writings which may claim to be such. The latter, an admirable and still invaluable treatise, appeared as an article in Rudolf Wagner's Cyclopædia of Physiology, but is best known to English readers in its separate form, in a translation made by the late Professor Henfrey, with the author's sanction, issued by Van Voorst in 1852. A year or two later it was for a time understood, to the great satisfaction of botanists, that Mohl had agreed to take a prominent part in the production of a general Manual of the Anatomy and Physiology of Plants; but his promise was soon withdrawn. For thirty years he was one of the editors of the *Botanische Zeitung;* but the editorial labor must have devolved mainly upon Schlechtendal and his successor, although occasional articles from Mohl's pen appeared as late as the year 1871. During that year his health became seriously impaired;

yet, as the new year advanced, apprehension disappeared. Upon Easter Monday he was apparently well, and so retired to nightly rest; in the morning he was found to have died in sleep.

Louis Alphonse de Brebisson, died at Falaise in Normandy, April 26, 1872, at the age of 74 years. Besides the Flora of his native province, which passed through four editions, and some good papers upon *Algæ*, he was distinguished for his collections and knowledge of *Diatomaceæ*.

Robert Wight, M.D., died at his residence, near Reading, England, May 26, 1872, at the age of 76 years. He was born in East Lothian, Scotland, educated at the Edinburgh High School, and professionally at Edinburgh University, where he took his medical degree in 1816. He went to India, the field of his botanical career and most useful administrative activity for forty years, in 1819. He was first assistant surgeon and afterward full surgeon of a native regiment in the E. India Company's service; but was soon transferred to the charge of the Botanic Garden at Madras, and finally to that of the important Cotton plantations at Coimbatoor. His earliest botanical contributions occupy a conspicuous place in Hooker's Botanical Miscellany, commencing in 1830, and in the continuations of that work under other names and firms. In 1834, after a temporary sojourn in his native city, appeared the first volume of a model flora, the *Prodromus Floræ Peninsulæ, Indiæ Orientalis*, by Dr. Wight and Mr. (afterwards Professor) Arnott, of which their successors in the field remarked, that it is the most able and valuable contribution to Indian botany which has ever appeared, and one which has few rivals in the whole domain of botanical literature. Dr. Wight returned to India immediately after the publication of this initial volume of the work, which was never continued. In India, assisted by native artists whom he had trained, he brought out two quarto volumes of "*Illustrations of Indian Botany*," with 182 colored plates; his *Spicilegium Nielgherrense*, of similar character, and finally his *Icones Plantarum Indiæ Orientalis*, in 6 volumes, with 2101 uncolored lithographic plates, and elaborate analysis, of unequal merit, many of them truly excellent but all wonderful, under the circumstances of their production. When he returned to England, nearly twenty years ago, his productive season, as it proved, was nearly over. But he distributed his collections with a liberal hand, as indeed he had always done, and in spite of a failing health enjoyed in a serene and happy old age the quiet country residence to which he had retired.

George Francis Reuter, the Director of the Botanic Garden at Geneva, Switzerland, for many years the companion in botanical journeys and investigation of M. Boisser, and the curator of his vast herbarium, died on the 22d of May last, at the age of about 55 years. He elaborated the *Orobanchaceæ* for De Candolle's *Prodromus*, published a Catalogue of the plants of the vicinity of Geneva, an Essay on the Vegetation of Castile, &c.; and was a man greatly esteemed by all who knew him.

Andreas S. Œrsted, Professor of Botany in the University of Copenhagen, died September 3d, at the age of 56 years. He began as a zoölogist, but since his return from his explorations in Costa Rica (1846–48), and his appointment to the chair of Botany in 1860, he has been one of the most active of Scandinavian botanists, and has treated with ability a great variety of subjects. In the Fungi, it was his interesting discovery that *Rœstelia* is a state of *Podisoma.*

Arthur Gris, an *aide-naturalist* at the Museum of Natural History, Paris, and one of the best botanical *elèves* of that establishment, died on the 18th of August last, at the early age of 42. He was associated with Professor Brongniart and was joint-author with him of several papers, mostly on new plants of New Caledonia. His independent publications were more numerous and important. Most of them related to anatomical and morphological topics. His thesis for the doctorate in sciences, in 1857, was founded on his microscopical investigation upon chlorophyl, which he was naturally led to undertake from the fact that his father, then deceased, had discovered that a chlorosed, or rather *dechlorosed*, plant was made green and vigorous under the action of salts of iron. One of his latest and most elaborate publications was a memoir upon the pith of plants, proving that this is much more diverse in structure than was supposed, and that the differences to a good extent coincided with ordinal characters.

Frederik Welwitsch, M.D., died in London, on the 20th of October last, in the 69th year of his age. He was a native of Carinthia; was educated at Vienna; was commissioned by the Würtemburg Unio Itineraria to collect the plants of the Azores and Cape Verd Islands; but on reaching Lisbon and finding good employment there, he made Portugal the field of his investigations, until, in 1850, he was sent by the Portuguese government to explore the natural history of its possessions on the west coast of Africa. His exploration of Angola and Benguela was rewarded by the discovery of more highly curious plants, probably, than any other that has been undertaken since Australia was opened to botanists; among them, and strangest of all, the genus which commemorates the discoverer, *Welwitschia mirabilis*, which Dr. Hooker, who described and illustrated it, does "not hesitate to consider the most wonderful, in a botanical point of view, that has been brought to light during the present century." Perhaps the limitation in the latter clause of the sentence is needless. This inhabits a most arid waste. In another district, under almost opposite conditions, Welwitsch had the good fortune to find the only Cactaceous plant indigenous out of America, viz., *Rhipsalis Cassytha*, and in a lake a new and most remote habitat of our *Brasenia peltata!* In his *Sertum Angolense*, a splendid memoir published by the Linnæan Society, with 25 plates, some of his most interesting discoveries are described; but the still unpublished portions of his collections must furnish most important contributions to the *Flora of Tropical Africa*, now in progress under the orders of the British Colonial department and the editorship of Prof. Oliver of Kew. It is to be hoped that they may be more fully available for this Flora than they have thus far been.

12. JOHN TORREY.—This great bereavement, which took place on the 10th of March, was announced in the last number of this Journal. For want of space the biographical notice is deferred to the June number. A. G.

13. *Sachs' Lehrbuch. 3d edition. Lehrbuch der Botanik nach dem gegenwärtigen-stand der Wissenschaft bearbeitet von* DR. JULIUS SACHS, Ord. Professor der Botanik in Würzburg. Dritte Auflage. Leipzig, 1873.—This treatise is divided into three books. The first is devoted to General Morphology of the Cell, Tissues, and Organs of Vegetation; the second to the subject of Special Morphology, and the elements of Systematic Botany. In the third, the author treats of Vegetable Physiology under the following heads: Molecular Force in Plants, Chemical Phenomena, General Conditions of Vegetable Life, Mechanics of Growth, Some phenomena of Motion, Sexuality, Origin of Forms.

The present edition has some important changes, the following subjects having been entirely re-written: *Hepaticæ*, *Lycopodiaceæ*, Action of Light, and Effect of Gravitation. Several other sections have received additions to the text, and the work has been brought down to the summer of 1872. It is a treatise of such importance that the English translation, now said to be in preparation, will be warmly welcomed. G. L. G.

14. *The Expression of the Emotions in Man and Animals;* by CHARLES DARWIN, M.A., F.R.S., etc. With photographic and other illustrations. 374 pp. 12mo. 1872. London: John Murray; New York, D. Appleton & Co.—Darwin here reviews the various facts with regard to the expressions of feelings and emotions in animals, gives explanations of their introduction and permanence, and enforces therewith his argument in support of the theory of natural selection. The work shows, like his other volumes, the laborious, faithful and deep thinking philosopher, whatever may be the final decision with regard to some of his deductions.

III. ASTRONOMY.

1. *On the variation in the diameter of the Sun;* by A. SECCHI.—Secchi has reduced a series of observations on the diameter of the sun, extending from July 12, 1871, to July 21, 1872, made by his assistant, P. Rosa. Chronographic transits of the sun were taken over 19 fixed threads and one movable thread, thus giving 20 transits for each limb of the sun. The probable error of an observation deduced from the 20 threads was 0″·31, and the minimum error was 0″·5 of arc. He thus obtained 187 reliable determinations, which were rendered comparable by having been first reduced to the mean solar distance. These observations corresponded to heliographic latitudes comprised between 0 and 26 degrees. While the probable error never exceeded 0″·5 of arc,

the isolated determinations often differed by 3, 4 or even 5 seconds. Secchi thinks that this difference cannot be attributed to accidental errors, for they existed during several consecutive days, and passed insensibly from one value to another; also, the comparison of these measures with analogous ones made at Palermo show variations sufficiently corresponding to those found at Rome, to prove the reality of these variations in the solar diameter.

From an examination of the curves of these variations, Secchi deduces that the diameter had minima values when the number of spots and protuberances were at the maxima. This result shows that there is a physical relation which merits a special study. The region between 20 and 23 degrees of heliographic latitudes affords the smallest diameters, and this is exactly the zone where the solar activity is the greatest, as resulted from the comparisons and extent of the protuberances and spots. Secchi thus provisionally accounts for the above relation. The border of the sun is not perfectly defined, the want of definition in its contour being, probably, due to the light of the chromosphere, which, very bright at its base, fuses to some extent with the photosphere. The ordinary diameter of the sun is composed of the diameter of the photosphere augmented by this brighter inferior layer, whose height equals 4 seconds, which his measurers assign as the difference between the semi-diameter observed and that of the ephemeridis. If this explanation be true, knowing that the chromosphere is sometimes more and sometimes less brilliant, it results that the solar diameter, thus augmented, will be found to diminish and to increase with the brilliancy of the chromosphere.—*Memorie della Società degli Spettroscopisti Italiani, Dispensa* 9^a, Sept. 1872. A. M. M.

IV. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *The Tyndall Endowment.*—Prof. TYNDALL, as is generally known, generously devoted the entire avails of the lectures he delivered in the United States to the cause of scientific training and for the advantage of scientific students. In his "Lectures on Light," just published by the Appletons, we have a statement (p. 190) of the money returns by his thirty-five lectures, as follows:

"Boston, six lectures, $1,500; Philadelphia, six lectures, $3,000; Baltimore, three lectures, $1,000; Washington, six lectures, $2,000; New York, six lectures, $8,500; Brooklyn, five lectures, $6,100; New Haven, two lectures, $1,000; making the total $23,100.

"Of this amount, the surplus above expenses, amounting to upward of $13,000, was conveyed by an article of trust to the charge of a committee, consisting of Prof. Joseph Henry, Gen. Hector Tyndall, and Prof. E. L. Youmans, who are authorized to expend the interest in aid of students who devote themselves to original researches."

Prof. Tyndall's first thought was to provide for the residence of such students at some German university, reserving to himself the choice of the institution, the candidate being selected by his Trustees. But he finally gave the Trustees a discretionary power to decide upon the place of study for the candidate whether in America or Europe.

Prof. Tyndall also gave $250 to the Yale Scientific Club in aid of original research.

2. *The Depths of the Sea. An account of the General Results of the Dredging Cruises of H. M. SS. "Porcupine" and "Lightning" during the summers of* 1868, 1869, *and* 1870; by C. WYVILLE THOMSON. 8vo, 527 pages, with 84 cuts and 8 maps and diagrams. New York and London: Macmillan & Co., 1873.—In this work the author has given in popular form a very interesting and somewhat detailed account of the several deep-sea dredging expeditions sent out by the English government in 1868, 1869, and 1870, under the scientific direction of the author, Dr. Wm. B. Carpenter, and Mr. J. Gwyn Jeffreys.

The principal zoölogical, physical, and chemical results of the expeditions are presented clearly and in a very readable form. Many of the more interesting and remarkable of the deep-sea animals discovered during these expeditions are illustrated by excellent figures, and many are also described, for the first time, in this work.

A full account is given of the equipment of the vessels, "Lightning" and "Porcupine," detailed for these expeditions, together with descriptions of the apparatus used in sounding, dredging, etc. One chapter is devoted to a history of deep-sea soundings, with descriptions and figures of the various kinds of apparatus employed. In another chapter a historical sketch of scientific dredging is given, with special reference to the deep-sea dredgings undertaken within a few years past.*

In this chapter the author has, by an unaccountable mistake or oversight, failed to do justice to the U. S. Coast Survey and to Mr. Pourtalès, by whom the first dredgings beneath the waters of the Gulf Stream were undertaken in 1867 and carried on with great success during three seasons. The author says (p. 277), "In the year 1868 Count L. F. de Pourtalès, one of the officers employed in the United States Coast Survey under Professor Peirce, commenced a series of deep dredgings across the Gulf Stream off the coast of Florida; which were continued in the following year, and were productive of most valuable results." He also quotes an extract from a letter written by Mr. Pourtalès to one of the editors and published in this Journal, giving a very brief account of the results of the second expedition (1868). But he nowhere alludes in any way to the previous expedition, in 1867, which antedates the first English expedition an entire year, although Mr. Pourtalès had published the very interesting and important results of that exploration in December, 1867, in the Bulletin of the Museum of Comparative Zoölogy, No. 6, together with descriptions of many of the new and remarkable animals obtained. These discoveries and the paper referred to were extensively noticed and discussed at that time by the scientific periodicals, both in this country and Europe, and were universally regarded as of the highest interest and value. Moreover, this pioneer expedition and the report upon it have been constantly referred to in all the subsequent publications upon the subject by Mr. Pourtalès, Mr. A. Agassiz, Mr. Theodore Lyman, and others, in the Catalogues and Bulletins of the Museum of Comparative Zoölogy and elsewhere.* The zoölogical results were also given in Günther's Zoölogical Record. It cannot, therefore, be supposed that Dr. Wyville Thompson and his colleagues were ignorant of this earliest exploration, or of its results. It has, moreover, been repeatedly referred to in English periodicals and in public addresses delivered and published in England.

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* The writer has already given in this Journal (II, xlix, p. 129, 1870) a sketch of the deep-sea explorations up to that date.

Nevertheless it is in no way referred to; not even in the published correspondence between Dr. Thomson, Dr. Carpenter, General Sabine, Mr. Romaine, and the secretary of the Royal Society, which preceded the organization of the first English expedition, in 1868. Yet it was generally believed, at the time, that the success of the American exploration had some influence upon the fitting out of that expedition. The first of the published correspondence is a letter from Dr. Thomson to Dr. Carpenter, dated May 30th, 1868, reciting the desirability of such explorations, suggesting the possible results [already in part realized by Pourtalès], and alluding to the important discoveries made by Dr. G. O. Sars, off the Loffoden Islands, but not mentioning Pourtalès or the United States Coast Survey.

One very important chapter is devoted to the deep-sea temperatures, with descriptions of the instruments, etc. A chapter is also devoted to the Gulf Stream. In this the various opinions and theories of previous writers are given and discussed, in the light of the numerous facts recently acquired concerning its phenomena.

It will be of interest to many to learn that the author does not sanction the direct-heat theory recently proposed and strongly urged by his colleague, Dr. Carpenter, to account for this and other ocean currents and a supposed general oceanic circulation, due to the differences in temperature and density between the tropical and polar waters. Dr. Thomson adopts the generally received theory, that the winds, and especially the trade-winds, furnish the motive power for ocean currents.

The last chapter is devoted to a discussion of the "continuity of the chalk." In this place the author has so modified his former views (or at least the statement of them) concerning this point that very few geologists will be disposed to oppose them. As he now explains his theory, viz: that a portion of the Atlantic basin has been occupied by a deep-sea from the Cretaceous to the present time, and that in this basin a chalk deposit has been continually forming ever since the Cretaceous period, or even earlier, under similar conditions, it is unobjectionable. But it is no more logical to say, on this account, that we may be regarded as "still living in the Cretaceous period," than it would be to say we are still living in the *Glacial period*, because glaciers still remain in certain localities, and have persisted since the Glacial period. That the abyssal fauna of the Atlantic has many relations with the ancient Cretaceous fauna of the same region is certain, from the researches of all the naturalists who have described the deep-sea forms; and in this country, Mr. Pourtalès and Mr. A. Agassiz have brought out these relations very strongly in respect to the corals and Echini, but these relations are only *generic*. No undoubtedly identical *species* have been discovered, unless among the Foraminifera.

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* A brief historical sketch of the Gulf Stream explorations is given by Pourtalès in the introduction to his "Deep Sea Corals," Illustrated Catalogue of the Museum of Comparative Zoölogy, No. IV, 1871; and in Petermann's Geographische Mittheilungen, Heft xi, 1870.

When we take into consideration the *totality of life*, both of the land and waters, in the Cretaceous and the present periods, the difference is enormously great, and the resemblances very few and exceptional. Many of the animals of Australia have very close relations with those of past geological periods, or in other words Tertiary and earlier types have persisted there, while in other regions they have been exterminated, or supplanted by other groups. Are we, on this account, to regard the Australian fauna as belonging to the Tertiary, Cretaceous, or Jurassic periods? Such instances of the persistence of certain ancient genera and families of animals and plants, in particular countries or under peculiar circumstances, are very numerous, and many of those that have long been known are quite as remarkable as those now brought to light from the deep-sea, but many of them have become too familiar to attract the attention that they deserve. A. E. V.

3. *The Challenger Expedition.* Letter from Prof. WYVILLE THOMPSON. (Nature, March 20.)—H. M. S. *Challenger* cast off from the jetty at Portsmouth, at 11.30 A. M. on December 21, with a low barometer. A strong southwesterly breeze was blowing, and the drum up; so that, especially in a season like the present, the prospect was not promising for the first few weeks of her voyage round the world.

The result justified the drum, and for a week we were knocking about the mouth of the Channel and the Bay of Biscay, making slow progress southward. It was perhaps as well to get a good shaking at first. It showed at once where there was a screw loose, and gave a chance to tighten it up. A sharp cyclone which caught the ship on her way from Sheerness to Portsmouth had already tested pretty fully the stowing of the apparatus, and although the *Challenger* rolls considerably when she is put to it, (over 35°), not a single instrument shifted, and not a glass was broken, either in the zoölogical workroom, or in the chemical laboratory. Just before we got to Lisbon the weather improved a little, and we got some soundings and took one or two trial hauls with the dredge.

After leaving Lisbon on January 12 the wind was again fresh, but between Lisbon and Gibraltar we made some important experiments, and found, among other things, that we could work easily and successfully with the common trawl down to 600 fathoms. I am now writing about 100 miles north of Madeira, and since leaving Gibraltar the weather, though at first breezy, has been on the whole fine. We have taken several successful navigative sounds at great depths, and we have trawled successfully at 2,125 fathoms, and recovered many interesting animal forms, several of them new to science, and others of extreme rarety and beauty. Still we must regard our work up to the present time as only tentative. The weather has been against us. It is altogether a new experiment to dredge from so large a ship, and it seems to present some special difficulties, or at all events to require some management. The weight of the ship is so great that there can be no "give and take" between it and the dredge, such as we have in the case of a smaller vessel. If there is any way on, the impulse to the dredge is irresistible, and seems to tend to jerk it off the ground. This difficulty can no doubt be met, but the only way of meeting it appears to be by using a length of rope greatly in excess of the depth—and having weights. A single dredging operation may thus occupy a great length of time, but in compensation we have the greater size and efficiency of this dredge. The few trials which we have already made have been all in the direction of improvement, and I have little doubt that under Captain Nares' skillful management what little difficulty is still felt will shortly disappear.

The *Challenger* is a spare-decked corvette of 2,000 tons displacement. This particular build gives her an immense advantage for her present purposes, as she has all the accommodation of a frigate, with the handiness and draught of water of a corvette. Sixteen of the eighteen 68-pounders which form the armament of the *Challenger* have been removed, and the main-deck is almost entirely set aside for the scientific work. The after-cabin is divided into two by a bulk-head, and two little rooms thus formed—still gay with mirrors, and pictures, and new chintz, and bright with home faces—are allotted to Captain Nares and myself. The fore cabin, a handsome room, 30 feet long by 12 feet wide, into which these private cabins open, the captain and I use as a sitting room, the port-end with its writing table and work table, and its book-cases packed with old home favorites, being appropriated to my use and that of my secretary, Mr. Wild; while the captain has arrangements at the starboard end of the same kind. Two sets of cabins have been especially built on the after-part of the main deck for this difficult part of the scientific work. On the port side a commodious zoölogical work-room is occupied by the naturalist of the civilian staff, while the chart room corresponds with it on the opposite side. Toward the middle of the main deck, on the port side, there is a dark room and a working room for the photographer, and on the starboard side Mr. Buchanan has his chemical and physical laboratory.

Nearly the whole of the fore-part of the main deck is occupied by the dredging and sounding gear, Mr. Seimens's photometric and thermometric apparatus, and the more cumbrous of our machines, such as the hydraulic pump, the aquarium, and other valuable articles, of which a detailed description will be given hereafter.

* * * * *

Dredging and sounding are carried on from the main-yard. A strong pennant is attached by a hook to the cap, and then by a tackle to the end of this yard. A compound arrangement of fifty-five of "Hodges' accumulators" is hung to the pennant, and beneath it a block through which the dredge rope passes. This arrangement appears to answer better than the old one of dredging from a derrick.

For the first two or three hauls in very deep water off the coast of Portugal, the dredge came up filled with the usual "Atlantic ooze," tenacious and uniform throughout, and the work of hours in sifting gave the very smallest possible result. We were extremely anxious to get some idea of the general character of the fauna, and particularly of the distribution of the higher groups; and after various suggestions for modifications of the dredge it was proposed to try the ordinary trawl. We had a compact trawl with a 15 ft. beam on board, and we sent it down off Cape St. Vincent at a depth of 600 fathoms. The experiment looked hazardous, but to our great satisfaction this trawl came up all right, and contained, with many of the larger invertebrata, several fishes. Two of these belonged to the genus *Macrourus*, and another of large size was unknown to us, approaching in many respects the genus *Mugil.* All the fishes were in a peculiar condition from the expansion of the air contained in their bodies. On their relief from the extreme pressure, their eyes especially had a singular appearance, protruding like great globes from their heads.

After this first attempt we tried the trawl several times at depths of 1,090, 1,525, and finally 2,125 fathoms, and always with success.

Several fishes, most of them allied to *Macrourus*, were added to the list. Several decapod crustaceans, and among the lower crustaceans, at 1,090 fathoms, a gigantic amphipod, of the family Hyperina, allied to *Phronima.* The eyes of this creature are very remarkable, extending in two great facetted lobes over the whole of the anterior part of the cephalothorax, like the eyes of *Aeglina* among Trilobites. This crustacean, which is three and a half inches in length, makes a splendid drawing, and reminds one of the old Eurypterids, is in process of description at the hands of Dr. von Willemoes Suhm.

Mollusca are very scarce in deep water, and our catches have hitherto been chiefly confined to such things as the species of *Nucula*, *Leda*, *Verticordia*, &c., familiar through the deep dredgings of the Porcupine. Among the molluscoids a haul in 1,525 fathoms gave us a lovely thing, a bryozoan forming, out of branches closely resembling those of *Acamarchis neritina*, a graceful cup, the bases of the branches united by a transparent stem between two and three inches high, like the barrel of a quill, or the stem of a claret glass. This genus, which presents a general character totally different from anything hitherto known among recent Bryozoa, I mean to dedicate to Captain Nares, as an early recognition of the confidence and esteem which he has already fully gained from the scientific staff. *Naresia cyathus* certainly re-calls, in the most singular way, the Cambrian *Dictyonema*, a form which I had, however, hitherto been inclined to refer to the Hydrozoa.

The Echinoderms have yielded some exceedingly interesting species to the trawl; among them several examples of the beautiful little urchins, of which one specimen was taken by Count Pourtales, in the Straits of Florida, and described by Alexander Agassiz under the name of *Salenia varispina.* It is undoubtedly a true *Salenia*, and to an advocate of the doctrine of the "continuity of the chalk," it is pleasant to see in the flesh this little beauty, which has hitherto been reckoned among the lost tribes.

Among the star-fishes two species of the genus *Hymenaster* have occurred, and the ophiurids are well represented chiefly by large examples of several species of the genus *Ophiomusium.*

All the hauls of the trawl, down to 2,125 fathoms, have yielded many specimens of a singnlar Holothurid, of which a description will shortly be published by Mr. Moseley. The animal is of a rich violet color. * * *

Sea-pens and Gorgoniæ have occurred frequently, always remarkable for their brilliant phosphorescence. Captain Maclear is giving special attention to this beautiful phenomenon. A *Mopsea*, which shone very brilliantly, gave a spectrum extending from the green well on into the red, while *Umbellularia* gave a very restricted spectrum sharply included between the lines *b* and D. Of this wonderfully rare sea-pen, we took with the trawl a very fine specimen, with a stem 3 ft. long, at a depth of 2,115 fathoms off Cape St. Vincent.

As usual in deep-sea work sponges preponderated, and the order has added several novelties, chiefly referable to the Ventriculite group, the Hexactinellidæ.

Some fine new species of *Aphrocallistes* came up along the coast of Portugal, and off St. Vincent; with many spicules and more or less mutilated examples of Hyalonema, two or three species in fair condition of a species of *Euplectella*, with spicules which I cannot distinguish from those of *Euplectella aspergillum* — the Venus flower-basket of the Philippines. The form of the two sponges is the same, but our own specimens are quite soft, the spicules not fused into a continuous siliceous network.

The physical and chemical observations will be fully detailed hereafter. The temperatures off the Coast of Portugal corresponded very closely with those taken in the *Porcupine* in 1870, and the *Shearwater* in 1871, below the first 100 fathoms, through which at this season the temperature is nearly uniform.

4. *New York Central Park.*— Second Annual Report of the Board of Commissioners. 253 pp. 8vo. New York. 1872.—The Central Park in New York is one of the public works of which

every American may well feel proud. For science, it has already accomplished something in its fine collection of trees and shrubs of North America and exotic, in its well ordered Meteorological Observatory, under the direction of Mr. Daniel Draper; its museum of Natural History and Menagerie and its noble engineering works. The chapter on "the disaster to trees," from the severe weather of March 13th–15th, 1872, will be read with interest as touching the possible extinction of certain species long regarded as most hardy, in localities where they have often withstood a greater fall of temperature, but never before such a remarkable coincidence of distinct agencies. A list of species and genera, of which 7,853 individuals were killed, in those two days, is given in this report.

The question of climatic change, and especially the clearing of land as affecting the rainfall, is discussed by Mr. Draper, in his report for 1872, by comparison of the New York observations with those of other Atlantic cities; and the conclusion is the same reached some years ago, by Prof. Loomis, in his classification of about ninety years observations, at New Haven, viz., that there has been no change in the average rainfall when long periods of time are compared, although the annual means may vary considerably. A further comparison is made of the number of days during which the Hudson river remains closed by ice, and it is shown that for the five decennial periods from 1817 to 1867, this river has remained closed for 92, 92, 94, 90 and 91 days respectively, of which the general mean is 91·142 days. The extremes were 136 days in 1842–43 and 47 days in 1841–42. The conclusion is, that during 50 years there has been no sensible change in the average climate of the Hudson river valley. Mr. Draper's meteorological report is illustrated with beautiful synoptic charts exhibiting to the eye these general facts, and also one for each of the twelve months of 1872. The latter show the movements of the barometer, thermometer and wind, for each day, in blue, red and green curves.

5. *Lists of Elevations in the portion of the United States west of the Mississippi River.* Collated and arranged by HENRY GANNETT. Miscellaneous publications, No. 1, of the U. S. Geol. Survey of the Territories, F. V. Hayden, Geologist-in-charge. Department of the Interior. 48 pp. 8vo, 1873.—These lists give the elevations of some thousands of places over the whole country west of the Mississippi, including the Rocky Mountain Region and the States and Territories to the west, making a pamphlet of great value to the geographer and very convenient for reference. The determinations that have been made along the routes of the various expeditions and geological surveys and the lines of railroad, about military posts, besides many from other sources, are here tabulated. A previous edition was issued in 1872 by Professor Cyrus Thomas, of the Survey, and is noticed in vol. iv, p. 246, of this Journal. This has been much enlarged by Mr. Gannett. It is proposed to issue an edition annually, with such additions and corrections as

may be obtained. The publication is one of the very valuable results of the Hayden Exploring Expedition.

6. *Recent discussions in Science, Philosophy and Morals;* by HERBERT SPENCER. New and enlarged edition. New York: D. Appleton & Company. 1873. 16mo, pp. 349.—This interesting volume contains the following thirteen essays: 1. Morals and moral sentiments; 2. Origin of animal worship; 3, The classification of the sciences; 4. Postscript—Replying to criticisms; 5. Reasons for dissenting from the philosophy of Comte; 6. Of laws in general, and the order of their discovery; 7. The genesis of science; 8. Specialized administration; 9. What is electricity? 10. The constitution of the sun; 11. The collective wisdom; 12. Political Fetichism; 13. Mr. Martineau on Evolution.

The first six of these essays have grown out of the discussions called up by the appearance of the seventh, "The genesis of science," printed some seventeen years since, in the author's "Illustratiens of universal progress." They deserve and will receive, as they have done already, the careful consideration of all thoughtful minds. Readers of "Nature" will remember the essay on electricity in which the author considers the reasons for regarding all electrical phenomena as due to some kind of molecular motion, a proposition which no physicist will controvert.

All of Mr. Spencer's writings are marked by originality and are well adapted to excite thoughtful discussion even when they do not command general assent. B. S.

7. *A set of Wind and Current Charts for the Pacific, Atlantic and Indian Oceans* has just been issued from the Hydrographic office of the Admiralty.

8. *Half-hour recreations in Popular Science;* DANA ESTES, editor, Boston. No. 4 of this Series is a chapter on Spectrum Analysis discoveries, showing its application in microscopical research, and to discoveries in the physical constitution and movements of the heavenly bodies, from the works of Schillen, Young, Roscoe, Lockyer, Huggins and others; No. 6 contains Dr. Carpenter's two excellent lectures on the Unconscious Action of the Brain, and Epidemic Delusions; No. 7, the Geology of the Stars, by Prof. A. Winchell.

9. *Erratum in Sang's Logarithms.*—Mr. N. Willey sends us the following correction to Sang's tables, viz: page 131, for the logarithm of 82885 read 9184759, instead of 9185759.

OBITUARY.

BARON LIEBIG.—The eminent chemist, Baron Justus Liebig, died at Munich on the 10th of April, aged seventy years.

APPENDIX.

Notice of New Tertiary Mammals; by O. C. MARSH.

IN addition to the extinct Mammals already described by the writer, the Museum of Yale College contains some interesting remains of this group from the various Tertiary deposits of the Rocky Mountain region. Not a few of these specimens are new to science, and some of the more important are here described.

Orohippus agilis, sp. nov.

Additional specimens of this genus fully justify its separation from *Anchitherium*, and likewise show that it holds a most interesting intermediate position between that genus and the less specialized mammals of the *Palæotherium* type. The genus differs essentially from *Anchitherium* in having four functional digits in the manus, in having the first premolar nearly as large as the second, and in the absence of an antorbital fossa. The skull is elongated, and equine in its proportions. The orbit was not enclosed behind. There were three upper true molars, and four premolars. The radius and ulna were separate, and the latter bone is stouter than in *Anchitherium*.

The present species differs from *Orohippus pumilus* Marsh,[*] in having the inner cones of the upper molars more nearly of equal size, and each with a distinct basal ridge. The remains preserved indicate, moreover, a somewhat larger animal, which nearly equalled a fox in size.

Measurements.

Space occupied by upper molar series,	49· mm.
Space occupied by upper true molars,	23·
Antero-posterior diameter of penultimate upper molar,	8·
Transverse diameter,	9·5
Transverse diameter of distal end of humerus,	20·
Transverse diameter of proximal end of radius,	17·
Transverse diameter of distal end on articulation,	12·4
Transverse diameter of distal end of ulna,	5·6
Length of third metacarpal,	55·5

The known remains of this species are all from the Eocene of Wyoming.

Colonoceras agrestis, gen. et sp. nov.

In its cranial characters and dentition, this genus resembles most nearly *Hyrachyus* Leidy, and *Helaletes* Marsh. It differs especially from these genera, so far as they are known, in the presence of a pair of dermal horns on the nasal bones, which were strengthened to support them. These horns were placed opposite each other, and their position, in a nearly perfect skull in the Yale Museum, is indicated by two rugosities, which have their surfaces marked by radiating lines. In the present species, which was about as large as a sheep, the horns were widely divergent.

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[*] This Journal, vol. iv, p. 207, Sept., 1872.

Measurements.

Space occupied by seven teeth in upper molar series,....	77· mm.
Extent of three true molars,......	41·
Distance between orbits,...........................	62·
Distance between apices of horn rugosities,............	27·
Length of frontals on median suture,...	62·
Expanse of occipital condyles,.......................	40·

The remains of this species at present known are from the Eocene of Wyoming.

Dinoceras lucaris, sp. nov.

This genus may be distinguished from *Tinoceras* Marsh (*Eobasileus* Cope), by the anterior position of the maxillary horns, by the elevated parietal crests, by the short and arched diastema, and by the compressed and trenchant canine tusks. From *Uintatherium* Leidy, so far as that genus is at present known, *Dinoceras* differs in the position of the occipital condyles, in the more anterior position of the posterior horns, and in the last upper molar, which lacks the external cone between the two transverse ridges, and has a second smaller tubercle behind the posterior ridge.

The present species, which may provisionally be referred to *Dinoceras*, differs from *D. mirabilis* Marsh,* aside from its larger size, in the structure of the upper molars. The penultimate has the inner posterior tubercle double, and the last true molar has a tubercle in the angle of the transverse crests, and also lacks the second posterior tubercle. The basal ridge is continuous on the inner side of each of the three upper premolars.

Measurements.

Space occupied by upper molar series,................	157· mm.
Extent of last three upper molars,....................	93·
Antero-posterior diameter of last upper molar,..........	35·5
Transverse diameter through posterior crest,...........	38·

The locality and geological horizon of these remains are essentially the same as those of the preceding species.

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* This Journal, vol. iv, p. 343, and vol. v, p. 117.

Oreodon occidentalis, sp. nov.

An interesting species of *Oreodon* occurs in the Miocene of Oregon, in the same deposits with *O. superbus* Leidy. It resembles *Oreodon Culbertsoni* in most of its cranial characters, but differs materially in the large auditory bulla, which is several times the size of the postglenoid process. The species is smaller than *O. major*, and has the frontals between the orbits more depressed, and the antorbital fossa deeper.

In comparing the various species of *Oreodon* some new points in the structure of this genus were observed. The dentition of all is essentially the same, the formula being as follows:—Incisors $\frac{3}{3}$, canines $\frac{1}{1}$, premolars $\frac{4}{4}$, molars $\frac{3}{3} \times 2 = 44$. The caniniform tooth of the lower jaw is clearly the first premolar, as Dr. Gill has stated. The metacarpals are slender, and those in *O. Culbertsoni* are about twice as long as those in *Dicotyles torquatus*. The first is wanting. The third and fourth are nearly equal in size, and had their coadapted faces immovably united by cartilage. The second and fifth are both well developed. The navicular and cuboid bones were loosely coössified, or separate. The phalanges are much more slender than in the Peccaries.

The following are some of the dimensions of a large specimen of *Oreodon occidentalis*.

Measurements.

Space occupied by last three upper molars,	45·5 mm.
Antero-posterior diameter of last upper molar,	15·5
Extent of last three lower molars,	53·
Distance between outer faces of postglenoid processes,	77·
Length of frontals on median suture,	54·
Vertical diameter of auditory bulla,	22·

The type specimen of this species was presented to the Museum of Yale College by Rev. Thomas Condon, who has done so much for the palæontology of Oregon. Other specimens were collected by the Yale party in the autumn of 1871.

Rhinoceros annectens, sp. nov.

There are two well-marked species of *Rhinoceros* represented in the Yale collections from the Miocene of Oregon. One of these Dr. Leidy has called *R. pacificus*;* the other appears to be undescribed. It was apparently about half the bulk of the former species, which it resembles in some of its dental characters. In the upper molars, however, the transverse crests approach each other much more nearly, and in the true molars preserved they are united, thus dividing the interposed valley.

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* Proceedings Philadelphia Academy, 1872, p. 248.

The basal ridge, also, is much less developed on the inner side of the upper molars. Upper incisors were present, and one of the lateral ones was greatly compressed, and its crown very short, as in the existing *R. Javanicus.*

Measurements.

Antero-posterior diameter of penultimate upper molar,...	27· mm.
Transverse diameter,..............................	36·
Antero-posterior diameter of first upper true molar,......	26·
Transverse diameter,..............................	34·5
Antero-posterior diameter of first upper premolar,........	20·6
Transverse diameter,..............................	17·
Antero-posterior diameter of upper incisor,..............	21·
Transverse diameter,..............................	7·6

The remains on which this species is mainly based were found, in November, 1871, in the John Day Valley, Oregon, by Mr. G. G. Lobdell of the Yale party.

Rhinoceros Oregonensis, sp. nov.

A second new species of this genus, much larger than either of the Miocene species, is indicated by portions of several individuals which were found by the Yale party, in 1871, in the Pliocene deposits of Oregon. One of these specimens is a penultimate upper molar which is quite characteristic, and differs widely from the corresponding tooth in any of the known species. At the union of the transverse posterior ridge with the outer cusp, there is a deep cavity, nearly circular, and enclosed by a vertical cylinder of enamel. The anterior crest, also, is divided, a strong branch being sent inward and backward from the posterior side into the main transverse valley.

Measurements.

Antero-posterior diameter of penultimate upper molar,..	41· mm.
Transverse diameter (approximate),..................	48·
Transverse diameter of circular cavity,................	6·5
Distance from center of cavity to front margin of tooth,.	21·

This species appears to have been about two thirds the size of *R. crassus* Leidy, from essentially the same geological horizon.

Yale College, New Haven, April 24th, 1873.

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[THIRD SERIES.]

ART. XLIV.—JOHN TORREY: *A Biographical Notice.*

THE following article forms a part of the Annual Report by the Council to the American Academy of Arts and Sciences, before which it was read at the meeting on the 8th of April, ult. This accounts for the form in which the biography is cast, and for the exclusion of many details and personal particulars which otherwise would naturally have found a place in it. It is the President of the American Academy rather than the companion and friend of many years who writes; yet the narrative must needs take tone and color from the intimate association of the writer with the subject of it. A. GRAY.

JOHN TORREY, M.D., LL.D., died at New York, on the 10th of March, 1873, in the 77th year of his age. He has long been the chief of American botanists, and was at his death the oldest, with the exception of the venerable ex-president of the American Academy (Dr. Bigelow), who entered the botanical field several years earlier, but left it to gather the highest honors and more lucrative rewards of the medical profession, about the time when Dr. Torrey determined to devote his life to scientific pursuits.

The latter was of an old New England stock, being, it is thought, a descendant of William Torrey, who emigrated from

Combe St. Nicholas, near Chard, in Somersetshire, and settled at Weymouth, Massachusetts, about the year 1640.*

His grandfather, John Torrey, with his son, William, removed from Boston to Montreal at the time of the enforcement of the "Boston Port bill." But neither of them was disposed to be a refugee. For the son, then a lad of 17 years, ran away from Canada to New York, joined his uncle, Joseph Torrey, a Major of one of the two light infantry regiments of regulars (called Congress's own) which were raised in that city: was made an ensign, and was in the rear-guard of his regiment on the retreat to White Plains; served in it throughout the war with honor, and until at the close he re-entered the city upon "Evacuation Day," when he retired with the rank of Captain. Moreover, the father soon followed the son and became quartermaster of the regiment. Captain Torrey, in 1791, married Margaret Nichols, of New York.

The subject of this biographical notice was the second of the issue of this marriage, and the oldest child who survived to manhood. He was born in New York, on the 15th of August, 1796. He received such education only as the public schools of his native city then afforded, and was also sent for a year to a school in Boston. When he was 15 or 16 years old his father was appointed Fiscal Agent of the State Prison at Greenwich, then a suburban village, to which the family removed.

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* In some notes furnished by a member of the family, the descent is endeavored to be traced through the eldest of the five sons who survived their parent, namely, Samuel, who came with him from England, became a minister of the gospel, and had the unprecedented honor of preaching three election sermons (in 1674, 1683, and 1695), as well as of having three times declined the presidency of Harvard College (after Hoar, after Oakes, and after Rogers). Although educated at the College, he was not a graduate, because he left it in 1650, after three years residence, just when the term for the A.B. degree was lengthened to four years. The tradition has it, that, "at the prayer meetings of the students, he was generally invited to make the concluding prayer,"—for which an obvious reason suggests itself,—for "such was his devotion of spirit that, after praying for two hours, the regret was that he did not continue longer." Students of the present day are probably less exacting.

The desire to claim a descent through so eminent a member of the family is natural. But our late venerable associate, Mr. Savage, in his Dictionary of early New England families, states that he could not ascertain that Samuel had any children.

At this early age he chanced to attract the attention of Amos Eaton, who soon afterwards became a well-known pioneer of natural science, and with whom it may be said that popular instruction in natural history in this country began. He taught young Torrey the structure of flowers and the rudiments of botany, and thus awakened a taste and kindled a zeal which were extinguished only with his pupil's life. This fondness soon extended to mineralogy and chemistry, and probably determined the choice of a profession. In the year 1815, Torrey began the study of medicine in the office of the eminent Dr. Wright Post, and in the College of Physicians and Surgeons, in which the then famous Dr. Mitchill and Dr. Hosack were professors of scientific repute; he took his medical degree in 1818; opened an office in his native city, and engaged in the practice of medicine with moderate success, turning the while his abundant leisure to scientific pursuits, especially to botany. In 1817, while yet a medical student, he reported to the Lyceum of Natural History—of which he was one of the founders—his Catalogue of the Plants growing spontaneously within thirty miles of the city of New York, which was published two years later; and he was already, or very soon after, in correspondence with Kurt Sprengel and Sir James Edward Smith abroad, as well as with Elliot, Nuttall, Schweinitz, and other American botanists. Two mineralogical articles were contributed by him to the very first volume of the American Journal of Science and Arts (1818–1819), and several others appeared a few years later, in this and in other Journals.

Elliott's sketch of the Botany of South Carolina and Georgia was at this time in course of publication, and Dr. Torrey planned a counterpart systematic work upon the botany of the Northern States. The result of this was his "Flora of the Northern and Middle Sections of the United States, i. e., north of Virginia,"—which was issued in parts, and the first volume concluded in the summer of 1824. In this work Dr. Torrey first developed his remarkable aptitude for descriptive botany, and for the kind of investigation and discrimination, the tact and acumen, which it calls for. Only those few,—now, alas, very few,—surviving botanists who used this book through the following years can at all appreciate its value and influence. It was the fruit of those few but precious years which, seasoned with pecuniary privation, are in this country not rarely vouchsafed to an investigator, in which to prove his quality before he is haply overwhelmed with professional or professorial labors and duties.

In 1824, the year in which the first volume (or nearly half) of his Flora was published, he married Miss Eliza Robinson Shaw, of New York, and was established at West Point, having been chosen Professor of Chemistry, Mineralogy and Geology in the United States Military Academy. Three years later he exchanged this chair for that of Chemistry and Botany (practically that of Chemistry only, for Botany had already been allowed to fall out of the medical curriculum in this country) in the College of Physicians and Surgeons, New York, then in Barclay Street. The Flora of the Northern States was never carried further; although a "Compendium," a pocket volume for the field, containing brief characters of the species which were to have been described in the second volume, along with an abridgement of the contents of the first, was issued in 1826. Moreover, long before Dr. Torrey could find time to go on with the work, he foresaw that the natural system was not much longer to remain, here and in England, an esoteric doctrine, confined to profound botanists, but was destined to come into general use and to change the character of botanical instruction. He was himself the first to apply it in this country in any considerable publication.

The opportunity for this, and for extending his investigations to the great plains and the Rocky Mountains on their western boundary, was furnished by the collections placed in Dr. Torrey's hands by Dr. Edwin James, the botanist of Major Long's expedition in 1820. This expedition skirted the Rocky Mountains belonging to what is now called Colorado Territory, where Dr. James, first and alone, reached the charming alpine vegetation, scaling one of the very highest summits, which from that time and for many years afterward was appropriately named James' Peak; although it is now called Pike's Peak, in honor of General Pike, who long before had probably seen, but had not reached it.

As early as the year 1823 Dr. Torrey communicated to the Lyceum of Natural History descriptions of some new species of James's collection, and in 1826 an extended account of all the plants collected, arranged under their natural orders. This is the earliest treatise of the sort in this country, arranged upon the natural system; and with it begins the history of the botany of the Rocky Mountains, if we except a few plants collected early in the century by Lewis and Clark, where they crossed them many degrees farther north, and which are recorded in Pursh's Flora. The next step in the direction he was aiming was made in the year 1831, when he superintended an American reprint of the first edition of Lindley's Introduction to the Natural System of Botany, and appended a catalogue of the North American genera arranged according to it.

Dr. Torrey took an early and prominent part in the investigation of the United States species of the vast genus *Carex*, which has ever since been a favorite study in this country. His friend, von Schweinitz, of Bethlehem, Penn., placed in his hands and desired him to edit, during the author's absence in Europe, his Monograph of North American Carices. It was published in the Annals of the New York Lyceum, in 1825, much extended, indeed almost wholly rewritten, and so much to Schweinitz's satisfaction that he insisted that this classical Monograph "should be considered and quoted in all respects as the joint production of Dr. Torrey and himself." Ten or eleven years later, in the succeeding volume of the Annals of the New York Lyceum, appeared Dr. Torrey's elaborate Monograph of the other North American Cyperaceæ, with an appended revision of the Carices, which meanwhile had been immensely increased by the collections of Richardson, Drummond, &c., in British and Arctic America. A full set of these was consigned to his hands for study (along with other important collections), by his friend Sir Wm. Hooker, upon the occasion of a visit which he made to Europe in 1833. But Dr. Torrey generously turned over the Carices to the late Professor Dewey, whose rival Caricography is scattered through forty or fifty volumes of the American Journal of Science and Arts; and so had only to sum up the results in this regard, and add a few southern species at the close of his own Monograph of the order.

About this time, namely in the year 1836, upon the organization of a geological survey of the State of New York upon an extensive plan, Dr. Torrey was appointed Botanist, and was required to prepare a Flora of the State. A laborious undertaking it proved to be, involving a heavy sacrifice of time, and postponing the realization of long-cherished plans. But in 1843, after much discouragement, the Flora of the State of New York, the largest if by no means the most important of Dr. Torrey's works, was completed and published, in two large quarto volumes, with 161 plates. No other State of the Union has produced a Flora to compare with this. The only thing to be regretted is that it interrupted, at a critical period, the prosecution of a far more important work.

Early in his career Dr. Torrey had resolved to undertake a general flora of North America, or at least of the United States, arranged upon the natural system, and had asked Mr. Nuttall to join him, who, however, did not consent. At that time, when little was known of the regions west of the valley of the Mississippi, the ground to be covered and the materials at hand were of comparatively moderate compass; and in aid of the northern part of it, Sir William Hooker's Flora of British America—founded upon the rich collections of the Arctic explorers, of the Hudson's Bay Company's intelligent officers, and of such hardy and enterprising pioneers as Drummond and Douglas,—was already in progress. At the actual inception of the enterprise, the botany of Eastern Texas was opened by Drummond's collections, as well as that of the coast of California by those of Douglas, and afterward those of Nuttall. As they clearly belonged to our own phyto-geographical province, Texas and California were accordingly annexed botanically before they became so politically.

While the field of botanical operations was thus enlarging, the time which could be devoted to it was restricted. In addition to his chair in the Medical College, Dr. Torrey had felt obliged to accept a similar one at Princeton College, and to all was now added, as we have seen, the onerous post of State Botanist. It was in the year 1836 or 1837 that he invited the writer of this notice—then pursuing botanical studies under his auspices and direction—to become his associate in the Flora of North America. In July and in October, 1838, the first two parts, making half of the first volume, were published. The great need of a full study of the sources and originals of the earlier-published species was now apparent; so, during the following year, his associate occupied himself with this work in the principal herbaria of Europe. The remaining half of the first volume appeared in June, 1840. The first part of the second volume followed in 1841; the second in the spring of 1842; and in February, 1843, came the third and the last; for Dr. Torrey's associate was now also immersed in professorial duties and in the consequent preparation of the works and collections which were necessary to their prosecution.

From that time to the present the scientific exploration of the vast interior of the continent has been actively carried on, and in consequence new plants have poured in year by year in such numbers as to overtask the powers of the few working botanists of the country, nearly all of them weighted with professional engagements. The most they could do has been to put collections into order in special reports, revise here and there a family or a genus monographically, and incorporate new materials into older parts of the fabric, or rough-hew them for portions of the edifice yet to be constructed. In all this Dr. Torrey took a prominent part down almost to the last days of his life. Passing by various detached and scattered articles upon curious new genera and the like, but not forgetting three admirable papers published in the Smithsonian Contributions to Knowledge (Plantæ Fremontianæ, and those on *Batis* and *Darlingtonia*), there is a long series of important, and some of them very extensive, contributions to the reports of government explorations of the western country,—from that of Long's expedition already referred to, in which he first developed his powers, through those of Nicollet, Fremont, and Emory, Sitgreaves, Stansbury, and Marcy, and those contained in the ampler volumes of the Surveys for Pacific Railroad routes, down to that of the Mexican Boundary, the botany of which forms a bulky quarto volume, of much interest. Even at the last, when he rallied transiently from the fatal attack, he took in hand the manuscript of an elaborate report on the plants collected along our Pacific coast in Admiral Wilkes's celebrated expedition, which he had prepared fully a dozen years ago, and which (except as to the plates) remains still unpublished through no fault of his. There would have been more to add, perhaps of equal importance, if Dr. Torrey had been as ready to complete and publish, as he was to investigate, annotate and sketch. Through undue diffidence and a constant desire for a greater perfection than was at the time attainable, many interesting observations have from time to time been anticipated by other botanists.

All this botanical work, it may be observed, has reference to the Flora of North America, in which, it was hoped, the diverse and separate materials and component parts, which he and others had wrought upon, might some day be brought together in a completed system of American botany.

It remains to be seen whether his surviving associate of nearly forty years will be able to complete the edifice. To do this will be to supply the most pressing want of the science, and to raise the fittest monument to Dr. Torrey's memory.

In the estimate of Dr. Torrey's botanical work, it must not be forgotten that it was nearly all done in the intervals of a busy professional life; that he was for more than thirty years an active and distinguished teacher, mainly of chemistry, and in more than one institution at the same time; that he devoted much time and remarkable skill and judgment to the practical applications of chemistry, in which his counsels were constantly sought and too generously given; that when, in 1857, he exchanged a portion, and a few years later the whole, of his professional duties for the office of U. S. Assayer, these requisitions upon his time became more numerous and urgent.* In addition to the ordinary duties of his office, which he fulfilled to the end with punctilious faithfulness (signing the last of his daily reports upon the very day of his death, and quietly telling his son and assistant that it would not be necessary to bring him any more), he was frequently requested by the head of the Treasury Department to undertake the solution of difficult problems, especially those relating to counterfeiting, or to take charge of some delicate or confidential commission, the utmost reliance being placed upon his skill, wisdom, and probity.

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* It ought to be added, that, when the Government Assay Office at New York was established, the Secretary of the Treasury selected Dr. Torrey to be its Superintendent,—which would have given to the establishment the advantage of a scientific head. But Dr. Torrey resolutely declined the less laborious and better paid post, and took in preference one the emoluments of which were much below his worth and the valuable extraneous services he rendered to the Government,—simply because he was unwilling to accept the care and responsibility of treasure.

In two instances these commissions were made personally gratifying, not by pecuniary payment, which, beyond his simple expenses, he did not receive, but by the opportunity they afforded to recruit failing health and to gather floral treasures. Eight years ago he was sent by the Treasury Department to California by way of the Isthmus; and last summer he went again across the continent, and in both cases enjoyed the rare pleasure of viewing in their native soil, and plucking with his own hands, many a flower which he had himself named and described from dried specimens in the herbarium, and in which he felt a kind of paternal interest. Perhaps this interest culminated last summer, when he stood on the flank of the lofty and beautiful snow-clad peak to which a grateful former pupil and ardent explorer, ten years before, gave his name, and gathered charming alpine plants which he had himself named forty years before, when the botany of the Colorado Rocky Mountains was first opened. That age and fast-failing strength had not dimmed his enjoyment, may be inferred from his remark when, on his return from Florida the previous spring, with a grievous cough allayed, he was rallied for having gone to seek Ponce de Leon's fountain of Youth. "No," said he, "give me the fountain of Old Age. The longer I live, the more I enjoy life." He evidently did so. If never robust, he was rarely ill, and his last sickness brought little suffering and no diminution of his characteristic cheerfulness. To him, indeed, never came the "evil days" of which he could say, "I have no pleasure in them."

Evincing in age much of the ardor and all of the ingenuousness of youth, he enjoyed the society of young men and students, and was helpful to them long after he ceased to teach,— if, indeed, he ever did cease. For, as Emeritus Professor in Columbia College (with which his old Medical School was united), he not only opened his herbarium, but gave some lectures almost every year, and as a trustee of the college for many years he rendered faithful and important service. His large and truly invaluable herbarium, along with a choice botanical library, he several years ago made over to Columbia College, which charges itself with its safe preservation and maintenance.

Dr. Torrey leaves three daughters, a son, who has been appointed U. S. Assayer in his father's place, and a grandson.

This sketch of Dr. Torrey's public life and works, which it is our main duty to exhibit, would fall short of its object if it did not convey, however briefly and incidentally, some just idea of what manner of man he was. That he was earnest, indefatigable, and able, it is needless to say. His gifts as a teacher were largely proved and are widely known through a long generation of pupils. As an investigator, he was characterized by a scrupulous accuracy, a remarkable fertility of mind, especially as shown in devising ways and means of research, and perhaps by some excess of caution.

Other biographers will doubtless dwell upon the more personal aspects and characteristics of our distinguished and lamented associate. To them, indeed, may fittingly be left the full delineation and illustration of the traits of a singularly transparent, genial, delicate and conscientious, unselfish character, which beautified and fructified a most industrious and useful life, and won the affection of all who knew him. For one thing, they cannot fail to notice his thorough love of truth for its own sake, and his entire confidence that the legitimate results of scientific inquiry would never be inimical to the Christian religion, which he held with an untroubled faith, and illustrated, most naturally and unpretendingly, in all his life and conversation. In this, as well as in the simplicity of his character, he much resembled Faraday.

Dr. Torrey was an honorary or corresponding member of a goodly number of the scientific societies of Europe, and was naturally connected with all prominent institutions of the kind in this country. He was chosen into the American Academy in the year 1841. He was one of the corporate members of the National Academy at Washington. He presided in his turn over the American Association for the Advancement of Science; and he was twice, for considerable periods, President of the New York Lyceum of Natural History, which was in those days one of the foremost of our scientific societies. It has been said of him that the sole distinction on which he prided himself was his membership in the order of the Cincinnati, the only honor in this country which comes by inheritance.

As to the customary testimonial which the botanist receives from his fellows, it is fortunate that the first attempts were nugatory. Almost in his youth a genus was dedicated to him by his correspondent, Sprengel: this proved to be a *Clerodendron*, misunderstood. A second, proposed by Rafinesque, was founded on an artificial dismemberment of *Cyperus*. The ground was clear, therefore, when, thirty or forty years ago, a new and remarkable evergreen tree was discovered in our own Southern States, which it was at once determined should bear Dr. Torrey's name. More recently a congener was found in the noble forests of California. Another species had already been recognized in Japan, and lately a fourth in the mountains of Northern China. All four of them have been introduced and are greatly prized as ornamental trees in Europe. So that, all round the world, *Torreya taxifolia*, *Torreya Californica*, *Torreya nucifera*, and *Torreya grandis*—as well as his own important contributions to botany, of which they are a memorial—should keep our associate's memory as green as their own perpetual verdure.

ART. XLV.—*Contributions from the Sheffield Laboratory of Yale College.* No. XXVI.—*On a compact Anglesite from Arizona;* by GEO. J. BRUSH.

ASSOCIATED with some specimens of galena from Castle Dome District, Arizona, there is found a compact banded mineral very much resembling some of the varieties of wood-tin from Cornwall. Mr. John C. Trautwine, C.E., of Philadelphia, having observed this anomalous substance, sent some specimens of it to me for determination in October last. A pyrognostic examination made at that time showed the mineral to be a compact anglesite. Subsequently Mr. Trautwine kindly provided me with more specimens, and a quantitative examination by Mr. Samuel T. Tyson, of this laboratory, has confirmed the correctness of the first examination.

The compact anglesite occurs in banded layers, sometimes with a nucleus of unoxidized and perfectly bright cleavable galena; while in other specimens the galena has entirely disappeared and the bands are symmetrically arranged in continuous circular or elliptical lines, as so often seen in agate. The bands or layers next the galena are frequently almost black, fading from a dark brownish-gray to a light grayish-white at the point farthest from the nucleus of galena, and the outer layer is sometimes exteriorly coated with minute, almost microscopic, crystals of transparent colorless anglesite. The thickness of these layers of compact anglesite in the specimens examined was from one-half to one inch. The specific gravity of the light colored variety was about 6, while some of the dark mineral gave a density as high as 6·44. Hardness = 3.

Mr. Tyson digested the finely pulverized mineral with a strong solution of bicarbonate of soda for 24 hours, then filtered and washed thoroughly and determined the sulphuric acid by precipitation as a baryta salt. The residue produced by the action of the bicarbonate was boiled with acetic acid, and the insoluble portion was collected on a weighed filter. The lead was then thrown down from the acetic solution by sulphuric acid. Four analyses made by this method gave Mr. Tyson.

	Dark variety.		Light variety.	
	1	2	1	2
Oxide of lead,	72·53	72·62	72·34	72.53
Sulphuric acid,	26·43	26·33	26·29	26·28
Insoluble residue,	0·75	0·73	0.83	0·75
	99·71	99·68	99·46	99·56

A fire-assay of the light variety yielded Mr. Tyson 0·0578 per cent silver, 16·87 ounces per ton of 2,000 lbs., while the galena was found to contain 27·3 ounces per ton, thus proving that a considerable portion of the silver was lost in the process of oxidation. The insoluble residue in the dark variety was almost black, and a qualitative examination of it showed it to be chiefly sulphide of lead, while the residue from the light-colored mineral was nearly white and almost entirely insoluble in acids, and proved to consist mainly of clay.

Sheffield Laboratory, New Haven, April, 1873.

ART. XLVI.—*On some Results of the Earth's Contraction from cooling, including a discussion of the Origin of Mountains, and the nature of the Earth's Interior;* by JAMES D. DANA.

PART I.

PREPARATORY to a discussion of some questions connected with the earth's contraction, I here present a statement of the views which I have entertained with regard to the prominent results of this agency. They first appeared in 1846 and 1847, in volumes ii, iii and iv of the second series of this Journal, and were somewhat extended in 1856, in vol. xxii.* Full credit is given to earlier writers in connection with the articles referred to. The views are as follows:†—

1. The defining of the continental and oceanic areas began with the commencement of the earth's solidification at surface, as proved by the system of progress afterward.

2. The continental areas are the areas of least contraction, and the oceanic basins those of greatest, the former having earliest had a solid crust. After the continental part was thus stiffened, and rendered comparatively unyielding, the oceanic part went on cooling, solidifying, and contracting throughout; consequently it became depressed, with the sides of the depression somewhat abrupt. The formation of the oceanic basins and continental areas was thus due to "unequal radial contraction."‡

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* Volume ii, 385; iii, 94, 176, 380: iv, 88; xxii, 305, 335.

† I may add in this place that a sight of Mädler's chart of the Moon in 1846, six years after my visit to the crater of Kilauea, in the Wilkes Exploring Expedition, prompted to the first of the articles on the subject—that on the Volcanoes of the Moon (II, ii, 335, 1846)—in which the origin of continents and oceanic basins is considered. The most important of Prevost's papers, on the origin of mountains had been published six years before, but I knew nothing of his views until after my paper was ready for publication, as I remark in a paragraph near its close.

‡ The principle thus expressed by Prof. LeConte in volume iv of this Journal, (1872,) does not differ essentially from my old view, except that it is connected with the idea of a solid globe. Prof. LeConte, on p. 466 of his article, attributes to me the opinion that the "sinking of sea bottoms, determined by interior contraction, is the [source of the] force by which continents are elevated." But I have never referred the origin of continents to such a cause, or to any other than that stated above.

Moreover, the elevation of mountains on the borders of continents I have attributed, not to "sinking sea-bottoms" merely, but to lateral pressure produced by contraction over continental as well as oceanic areas, that on the oceanic being made much the greatest, as stated beyond. My language is frequently ambiguous on this last point, because I speak of the oceanic as the "subsiding" areas. But the term is used relatively. In volume iii, on p. 179, (1847,) I observe that mountain elevations, occur "near the limit between the great contracting and the non-contracting (comparatively non-contracting) areas:" and in various places I describe the contraction as general. In my Manual of Geology, on page 732, I remark that the elevating "force acted most strongly from the oceanic direction," which was the idea throughout. I do not deny, however, that I have supposed too

3. The principal mountain chains are portions of the earth's crust which have been pushed up, and often crumpled or plicated, by the lateral pressure resulting from the earth's contraction.

4. (*a*) Owing to the lateral pressure* from contraction over both the continental and oceanic areas, and to the fact that the latter are the regions of greatest contraction and subsidence, and that their sides pushed, like the ends of an arch, against the borders of the continents, therefore, along these borders, within 300 to 1000 miles of the coast, a continent experienced its profoundest oscillations of level, had accumulated its thickest deposits of rocks, underwent the most numerous uplifts, fractures and plications, had raised its highest and longest mountain chains, and became the scene of the most extensive metamorphic operations, and the most abundant outflows of liquid rock.

And (*b*) since the most numerous and closest plications, the greatest ranges of volcanoes, the largest regions of igneous eruption and metamorphic action, exist on the *oceanic slope* of the border mountain chains, instead of the continental, therefore the lateral pressure acted most effectively in a direction *from* the ocean.

(*c*) Since these border features are vastly grander along that border of a continent which faces the largest ocean, therefore, the lateral pressure against the sides of a continent was most effective on the border of the largest oceanic basin, and for the two, the Pacific and Atlantic, was approximately proportioned to the extent of the basins; this being due to the fact that the oceanic were the subsiding areas, that is, those which contracted most, and that the larger area became the most depressed.

5. The oscillations of level that have taken place over the interior of North America, through the geological ages, have in some degree conformed in direction of axis to those of the border regions, all being parts fundamentally of two systems of movements, one dominantly in a direction northwestward or from the Atlantic, the other northeastward or from the Pacific.

6. Owing to the approximate uniformity of direction in the lateral thrust under these two systems through the successive ages, (a consequence of the isolated position of the continent between two oceanic basins, transverse to one another in axial direc-

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large a part of the lateral force to have come from the special contraction and consequent subsidence of the oceanic part of the globe.

Professor N. S. Shaler in 1866 (Proc. Boston N. H. Soc., x, 237, xi, 8, and Geol. Mag., v, 511) presented, *as original*, the idea that "mountain chains are only folds of the outer portion of the crust caused by the contraction of the lower regions of the outer shell;" and that "the subsidence of ocean floors would, by producing fractions and dislocations along shore lines, tend to originate mountain chains along sea-borders and approximately parallel to them;" which is essentially the view that LeConte attributes to me. These ideas are coupled with others respecting limitations of the action of contraction due to denudation and deposition, in which I have no share.

* In my papers in 1847 I used the terms lateral pressure, lateral force, tension, horizontal force, force acting tangentially, as synonyms. "Lateral pressure" was the term oftenest employed, and it was explained by reference to a Prince Rupert's drop. (See this Journ., II, iii, 96, etc.) The action appealed to was not in any way different from the "tangential thrust" of Mallet.

tion,) mountains *of different ages* on the same border, or part of a border, have approximately the *same trend*, and those *of the same age* on the opposite border—Pacific and Atlantic—have in general *a different and nearly transverse trend.* Hence, "one dial plate for the mountains of the world, such as Elie de Beaumont deduced mainly from European geology, will not mark time for America." (This Journ., II, iii, 398, 1847; xxii, 346, 1856.)

7. The features of the North American continent were to a great extent defined in pre-Silurian time, the course of the Azoic, from the Great Lakes to Labrador, being that of the Appalachians, and various ridges in the Rocky Mountains foreshadowings of this great chain, and so on in many lines over the continental surface; and thus its adult characteristics were as plainly manifested in its beginnings as are those of a vertebrate in a half-developed embryo.

8. Metamorphism of regions of strata has taken place only during periods of disturbance, or when plication and faults were in progress; all metamorphic regions being regions of disturbed and generally of plicated rocks.

The heat required for alteration came up from the earth's liquid interior. (This part of the view requires modification, while the other part, I believe, remains good.)

9. The volcanoes of the continental areas are mostly confined to the sea-borders, or the oceanic slope of the border mountain chains, not because of the vicinity of salt water, but because these were the regions of greatest disturbance and fractures through lateral pressure. Volcanoes are indexes of danger, never "safety-valves."

10. Earthquakes were a result of sudden fracturings and dislocations proceeding from lateral pressure. In vol. iii, p. 181, (1847,) occurs the remark: "We see that the lateral pressure exerted would be likely to dislocate," and in the next line, "such fissurings, whether internal or external, would cause shakings of the earth (*earthquakes*) of great violence, and in all periods of the earth's history, and it might be over a hemisphere at once."

Another important subject—that of the systems in the trends of feature lines over the globe—is discussed in the articles referred to; but I pass it by for the present.

I propose to bring the above principles under consideration with reference to making such changes as may now be necessary.

I take up, first, the question as to whether oscillations of level, that is, subsidences and elevations, have been made by the lateral pressure resulting from contraction, as is assumed in my writings on the subject and those of most other authors;—and how was the lateral thrust from the direction of the oceanic areas made to differ in its results from that from the opposite direction? After which I shall pass to the subjects of metamorphism, igneous eruptions, volcanoes, the earth's interior, and the origin of oceanic basins.

1. *Have subsidences been produced by lateral pressure?*

The theory of Professor James Hall, that the great subsidences of the globe have been made by the gravity of accumulating sediments, has been shown elsewhere* to be wholly at variance with physical law.

Another theory is presented by Prof. LeConte, in his recent paper in the last volume of this Journal, to which the reader is referred. Admitting, with Prof. Hall, that the mean thickness of the accumulations in the Appalachian region of Pennsylvania is 40,000 feet, and therefore that this is the measure of the gradual subsidence that attended their deposition, he shows that the temperature in the bottom deposits would have been, supposing the usual rate of increase downward (1° F. for 58 feet of descent), 800° F., and, at 10,000 feet, 230° F.; and he argues that hence there would have resulted below, first, "lithification and therefore increasing density, and therefore *contraction and subsidence pari passu* with the deposit;" next, or at a greater depth, "aqueo-igneous softening" or "melting," the temperature of 800° F. being "certainly sufficient to produce this result as well as metamorphism, and, during this process, the subsidence would probably continue;" and, in addition, the underlying strata on which the sediments were deposited would have participated in the aqueo-igneous fusion" and thus have added to the result.†

No other cause of the gradual subsidence than that here cited is appealed to.

Now *the whole of this contraction took place*, if any occurred, *in the underlying Archæan rocks* (Azoic, or Laurentian and Huronian); for in obtaining by measurement this thickness, 40,000 feet, the *contracted* rocks were measured.

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* This Journal, II, xlii, 210, 1866, III, v, 347, 1873; LeConte, ib., III, iv, 461, 1872

† The principal points in Prof. Hall's theory of mountains, published in 1859, (see p. 347, of this volume,) are:

1. Coast regions the courses of marine currents, and hence of deposited sediments.
2. The accumulation of sediments by their gravity gradually sink the crust, and thus a great thickness is attained; the rocks become solidified and sometimes crystallized below.
3. The continents afterward somehow raised—not the mountain regions separately.
4. Shaping of the mountains out of other sediments by denudation.
5. Metamorphism due to "motion," "fermentation," and a little heat; the heat coming up from below (the isogeothermal planes rising) in consequence of the increasing accumulations at surface.

In Prof. LeConte's theory (this Journ., III, iv, 345, 460, 1872):

1. The same as in Prof. Hall's.
2. As explained in the text above.
3. After an aqueo-igneous softening of the beds below, the lateral thrust from the earth's contraction pressed together the region of sedimentary accumulation, plicating and crushing the beds.
4. The elevation of mountains due solely to crushing and plication.
5. Metamorphism consequent on the heating derived by the rise of the isogeothermal planes.

The 40,000 feet of subsidence required was therefore wholly independent of contraction in the stratified sediments. But these underlying Archæan rocks were probably crystallized before the Paleozoic era began; for in New York and New Jersey they are in this condition, and they underlie the Silurian rocks unconformably; and the New Jersey Archæan or Highland region is but a northern part of that of Pennsylvania and Virginia. They would consequently have expanded with the heat instead of contracting. Even if not crystallized, they would have been well compacted under the enormous weight of 40,000 feet of strata, and no experiments on rocks that I have met with authorize the assumption that the ordinary law of expansion from heat would have been set aside.

For further argument on this point I refer to the subsidence in the Connecticut valley during the era of the Connecticut River sandstone (supposed to be Triassico-Jurassic). The thickness of rock produced in the era was probably about 4,000 feet, and this is the extent therefore of the registered subsidence. The sandstone strata, as is apparent in many places, rests on the upturned metamorphic rocks—gneiss, mica schist, etc.,—of Paleozoic or earlier age. As shown in the preceding paragraph, the contraction, under Prof. LeConte's principle, must have been confined to the underlying rocks; and since these are crystalline metamorphic schists, and the depth of sandstone was not sufficient to raise much the temperature within them (the rocks are in general little compacted and often feebly solidified), the heat ascending from below as accumulation went on above would have produced expansion instead of contraction.

Without further reference to facts, it is, I think, clear that the subsidence required could not be obtained by the method appealed to by Prof. LeConte. Whatever cause, in either of the above cases, occasioned the subsidence, it must have been one that could do its work in spite of opposition on the part of the heat in the rocks themselves or those below.

Another cause of local subsidence is local cooling beneath, accompanying the increasing accumulation of sediments. But this idea is too obviously absurd to require remark.

In the present state of science, then, no adequate cause of subsidence has been suggested apart from the old one of lateral pressure in the contracting material of the globe.

2. *Have elevations been produced directly by lateral pressure?*

The theory of Prof. Hall denies that mountains are a result of *local* elevations, or of any elevation apart from a general continental. This hypothesis I have elsewhere discussed.*

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* This Journal, II, xlii, 205, 252, and this volume, p. 347.

Prof. LeConte makes the elevation of mountains real, but, after explaining that the crushing effects of lateral thrust would necessarily cause a lengthening upward of the compressed strata (as in the compression of slate rocks attending the production of slaty cleavage), and thereby produce a large amount of actual elevation, arrives at the view, that there is no permanent elevation beyond what results from crushing. With crushing, in this action, plication is associated; but it should have a larger place than his words seem to give it (in all plication the rocks over a region being pressed into a narrower space, which could be done only by adding to the height), as it has performed ten-fold more work of this kind than crushing.

But are plication and crushing the only methods of producing, under lateral pressure, the actual elevations of mountain regions? Is there not real elevation besides?

In the later part of the Post-tertiary or Quaternary era, the region about Montreal was raised nearly 500 feet, as shown by the existence of sea-beaches at that height; and similiar evidence proves that the region about Lake Champlain was raised at the same time at least 300 feet, and the coast of Maine 150 to 200 feet. Hence the region raised was large. No crushing or plication of the upper rocks occurred, and none in the under rocks could well have taken place without exhibitions at surface; and this cause, therefore, cannot account for the elevation. The elevated sea-border deposits of the region are in general horizontal. This example is to the point as much as if a mountain had been made by the elevation.

But we have another example on a mountain scale, and one of many. Fossiliferous beds over the higher regions of the Rocky mountains are unquestioned evidence that a large part of this chain has been raised 8,000 to 10,000 feet above the ocean level since the Cretaceous era.* The Cretaceous rocks, to which these fossiliferous beds belong, were upturned in the course of the slowly progressing elevation, and so also were part of the Tertiary beds—for the elevation went forward through the larger part, or all, of the Tertiary era. But the local crushing or plication of these beds cannot account for the elevation, and no other crushing among the surface rocks of the mountains can be referred to this era. There may have been a crushing and crumpling of the nether rocks of the mountain. But it must also be admitted that there might have been, under tangential pressure, a bending of the strata without crushing, especially if there is beneath the earth's rind along the continental borders a region or layer of "aqueo-igneous fusion," such as Prof. LeConte recognizes.

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* The height of the Cretaceous (stratum No. 2 of the Upper Missouri Cretaceous) at Aspen, in Wyoming, is full 8,000 feet above tide level (Meek). Beds occur also in South Park, Colorado, the height of which is 8500 feet; and, according to Hayden, in the region of the Wind River Mountains, the beds have a height of 10,000 to 11,000 feet above the sea.

In the course of the geological history of the North American continent, there were many oscillations of level in the land. Portions that were raised above the sea-level in one era in another subsided again and sunk beneath it; and Prof. LeConte, in the course of his discussion, admits the existence of an elevated region along the Atlantic border which afterward disappeared. Had the elevation in the case of such oscillations been dependent on plication and crushing beneath, so complete a disappearance afterward would have been very improbable.

Such facts as the above appear to prove that elevatory movements have often been, like those of subsidence, among the direct results of lateral pressure. The facts are so well known and the demonstration so generally accepted as complete, that I have suspected that there is here an unintentional omission or oversight in Prof. LeConte's paper.

3. *Kinds and Structure of Mountains.*

While mountains and mountain chains all over the world, and low lands, also, have undergone uplifts, in the course of their long history, that are not explained on the idea that all mountain elevating is simply what may come from plication or crushing, the *component parts* of mountain chains, or those simple mountains or mountain ranges that are *the product of one process of making*—may have received, *at the time of their original making*, no elevation beyond that resulting from plication.

This leads us to a grand distinction in orography, hitherto neglected, which is fundamental and of the highest interest in dynamical geology; a distinction between—

1. A simple or *individual* mountain mass or range, which is the result of *one process of making*, like an individual in any process of evolution, and which may be distinguished as a *monogenetic* range, being *one in genesis;* and

2. A composite or *polygenetic* range or chain, made up of two or more monogenetic ranges combined.

The Appalachian chain—the mountain region along the Atlantic border of North America—is a *polygenetic* chain; it consists, like the Rocky and other mountain chains, of several *monogenetic* ranges, the more important of which are: 1. The Highland range (including the Blue Ridge or parts of it, and the Adirondacks also, if these belong to the same process of making) pre-Silurian in formation; 2. The Green Mountain range, in western New England and eastern New York, completed essentially after the Lower Silurian era or during its closing period; 3. The Allegbany range, extending from southern New York southwestward to Alabama, and completed immediately after the Carboniferous age.

The making of the Alleghany range was carried forward at first through a long-continued subsidence—a *geosynclinal** (not a *true* synclinal, since the rocks of the bending crust may have had in them many true or simple synclinals as well as anticlinals), and a consequent accumulation of sediments, which occupied the whole of Paleozoic time; and it was completed, finally, in great breakings, faultings and foldings or plications of the strata, along with other results of disturbance. The folds are in several parallel lines, and rise in succession along the chain, one and another dying out after a course each of 10 to 150 miles; and some of them, if the position of the parts which remain after long denudation be taken as evidence, must have had, it has been stated, an altitude of many thousand feet; and there were also faultings of 8,000 to 10,000 feet, or, according to Lesley, of 20,000 feet.† This is one example of a *monogenetic* range.

The Green Mountains are another example in which the history was of the same kind: first, a slow subsidence or geosynclinal, carried forward in this case during the Lower Silurian era or the larger part of it; and, accompanying it, the deposition of sediments to a thickness equal to the depth of the subsidence; finally, as a result of the subsidence and as the climax in the effects of the pressure producing it, an epoch of plication, crushing, etc. between the sides of the trough.

In the Alleghany range the effects of heat were mostly confined to solidification; the reddening of such sandstones and shaly sandstones as contained a little iron in some form;‡ the coking of the mineral coal; and probably, on the western outskirts where the movements were small, the distillation of mineral oil, through the heating of shales or limestones containing carbohydrogen material, and its condensation in cavities among overlying strata; with also some metamorphism to the eastward; while in the making of the Green Mountains, there was metamorphism over the eastern, middle, and southern portions, and imperfect metamorphism over most of the western side to almost none in some western parts.

Another example is offered by the Triassico-Jurassic region of the Connecticut valley. The process included the same stages in kind as in the preceding cases. It began in a geosynclinal of probably 4,000 feet, this much being registered by the thickness of the deposits; but it *stopped short of metamorphism*, the sandstones being only reddened and partially solidified; and *short of plication or crushing*, the strata being only tilted in a monoclinal manner 15° to 25°; it ended in numerous great longitudinal fractures, as a final catastrophe from the subsidence, out of which issued the trap (dolerite) that now makes Mt. Holyoke, Mt. Tom, and many other ridges along a range of 100 miles.*

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* From the Greek γῆ, *earth*, and *synclinal*, it being a bend in the earth's crust.

† See an admirable paper on these mountains by Professors W. B. and H. D. Rogers, in the Trans. Assoc. Amer. Geol. and Nat., 1840–42. J. P. Lesley gives other facts in his "Manual of Coal and its Topography," and in many memoirs in the Proceedings of the American Philosophical Society. A brief account is contained in the author's Manual of Geology.

‡ Oxide of iron produced by a wet process at a temperature even as low as 212° *F.* is the red oxide $Fe_2 O_3$, or at least has a red powder. (Am. Jour. Sci., II, xliv, *292.*)

These examples exhibit the characteristics of a large class of mountain masses or ranges. A geosynclinal accompanied by sedimentary depositions, and ending in a catastrophe of plications and solidification, are the essential steps, while metamorphism and igneous ejections are incidental results. The process is one that produces final stability in the mass and its annexation generally to the more stable part of the continent, though not stable against future oscillations of level *of wider range*, nor against denudation.

It is apparent that in such a process of formation elevation by direct uplift of the underlying crust has no necessary place. The attending plications may make elevations on a vast scale and so also may the shoves upward along the lines of fracture, and crushing may sometimes add to the effect; but elevation from an upward movement of the downward bent crust is only an incidental concomitant, if it occur at all.

We perceive thus where the truth lies in Professor LeConte's important principle. It should have in view alone *monogenetic* mountains and these only *at the time of their making*. It will then read, plication and shovings along fractures being made more prominent than crushing:

Plication, shoving along fractures and crushing are the true sources of the elevation that takes place *during the making* of geosynclinal monogenetic mountains.

And the statement of Professor Hall may be made right if we recognize the same distinction, and, also, reverse the order and causal relation of the two events, accumulation and subsidence; and so make it read:

Regions of monogenetic mountains were, previous, and preparatory, to the making of the mountains, areas each of a slowly progressing geosynclinal, and, *consequently*, of thick accumulations of sediments.

The prominence and importance in orography of the mountain individualities described above as originating through a geosynclinal make it desirable that they should have a distinctive name; and I therefore propose to call a mountain range of this kind a *synclinorium*, from *synclinal* and the Greek *ὄρος*, *mountain*.

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* This history is precisely that which I have given in my Manual of Geology, though without recognizing the parallelism in stages with the history of the Alleghanies.

This brings us to another important distinction in orographic geology—that of a second kind of monogenetic mountain. The *synclinoria* were *made through a progressing geosynclinal*. Those of the second kind, here referred to, were *produced by a progressing geanticlinal*. They are simply the upward bendings in the oscillations of the earth's crust—the geanticlinal waves, and hardly require a special name. Yet, if one is desired, the term *anticlinorium*, the correlate of *synclinorium*, would be appropriate. Many of them have disappeared in the course of the oscillations; and yet, some may have been for a time—perhaps millions of years—respectable mountains. The "Cincinnati uplift," extending southwestward from southern Ohio (about Cincinnati) into Tennessee, and referred by Newberry and others to the close of the Lower Silurian, was made at the same time, or nearly, with the Green Mountains; but, while the latter range is a synclinorium, the former is a geanticlinal or an anticlinorium, and it is one of the few (probably few) permanent monogenetic elevations of this kind over the earth's surface. There may possibly have been crumpling or crushing in the deep-seated rocks below which determined its permanence. As far as the Paleozoic rocks constituting it go, it is a *simple* synclinal; but it is really a synclinal of the earth's crust, and hence wholly distinct from ordinary synclinals, or those subordinate among the plications in a synclinorium, like the synclinals of the Alleghanies.

The geosynclinal ranges or synclinoria have experienced in almost all cases, since their completion, true elevation through great geanticlinal movements, but movements that embraced a wider range of crust than that concerned in the preceding geosynclinal movements, indeed a range of crust that comes strictly under the designation of a polygenetic mass. Thus the Connecticut valley sandstone beds, which must have been but little raised by the slight upturning they underwent at the epoch of their disturbance (since there was then neither plication nor crushing) are now seven hundred feet higher above the sea-level in Massachusetts than near New Haven, Conn.; and this is owing, not to denudation but to a subsequent elevation in which much of New England participated—a true geanticlinal uplift. So it has been the world over. The great uplift of the Rocky Mountain region of more than 8,000 feet, which began after the Cretaceous, had nothing to do, as I have said, with crushing or plication, although there was disturbance of the beds in certain local Cretaceous and Tertiary areas; it appears to have been a true geanticlinal elevation of the Rocky Mountain mass, itself mainly, if not wholly, a combination of synclinoria.

Geosynclinals and geanticlinals of low angle, like those of the present day, graduate insensibly into horizontal surfaces. The later oscillations in the world's history have taken in a vastly wider range of crust than those of early time. We cannot point to any geosynclinal in progress that is probably on the way to become the site of a new synclinorium. This comes from the fact already stated, that the completion of a synclinorium has generally consisted in the solidification as well as plication of the rocks, and the addition of the whole mountain region to the more stable portion of the earth's crust; and the further fact that this process has been often repeated in past time, until the crust has been so stiffened above, as well as below, that only feeble flexures of vast span are possible, even if the lateral pressure from contraction had not also declined in force.

4. *How was the lateral thrust from the direction of the ocean made to differ in its action or results from that from the opposite direction?*

The fact of a difference in the effects of the lateral thrust from the opposite directions, the oceanic and continental, is beyond question. The evidence may here be repeated.

The greatest of elevations as well as subsidences, and also of plications and igneous eruptions, have taken place on the continental borders or in their vicinity; they thus show that there is something peculiar along such regions. Again, the border mountains in North America are parallel to the axes of the adjoining oceans; and thereby at right angles, instead of parallel to one another. Again, the folds in the Appalachians are not symmetrical folds, but, instead, have one slope much steeper than the other, proving inequality in the action of lateral pressure from the continental and oceanic directions. Further, the larger ranges of uplifts and effects of heat occur on the *oceanic* slope of the principal border-mountain chain, instead of the continental slope, favoring the view that this lateral thrust was more effective in the direction from the ocean against the continents than in the opposite. Finally, there is the fact that the disturbances or effects of lateral thrust have been *very much the greatest* on the border of the *largest* oceans.

But has this greater effectiveness of lateral thrust from the direction of the ocean been due to a proportionally greater contraction and subsidence of the oceanic crust than the continental —the sinking causing the oceanic arch to press against the sides of the basin. I formerly made this the chief means of mountain lifting; and now, while not giving it so great prominence, I believe it to be a true cause. It is certain that the depressing of the ocean's bed, like the raising of the continental areas, has been in progress through the ages. The great principal rise of the continent and continental mountains took place after the Cretaceous period or during the Tertiary, and some of it even in the Quaternary; and this is almost positive demonstration that the bottoms of the oceans were tending downward cotemporaneously. It is not possible in the nature of contraction that it should have been all accomplished in these basins at the beginning of their existence—a point I shall further illustrate when discussing the nature of the earth's interior. Moreover, the mobile waters that occupy the oceanic depressions would have given important aid in the cooling of the underlying crust. It is to be noted, also, that the distance between the axis of the Appalachians in North America, and the opposite (African) side of the Atlantic is 4000 miles; and that between the axis of the Rocky Mountains and the opposite (Australian) coast of the Pacific is over 7000 miles, while between the axis of the Appalachians in Virginia and that of the Rocky Mountains in the same latitude, the distance is hardly 1500 miles. Hence the contraction was absolutely greatest over the oceanic areas, independently of any result from special causes; and if the generated pressure were not expended in uplifts over the oceanic areas themselves, it would have been in uplifts on its borders.

In addition to the above advantage which the oceanic areas have had in the making of border oscillations, the lower position of the oceanic crust, and the abruptness with which the sides fall off, give it an opportunity to push beneath the sides of the continents, and this would determine the production of such mountains and just such other effects of pressure, on the continental borders, as actually exist, even if contraction were equable over the globe, that is, were alike in rate over the oceanic and continental areas. It puts the oscillations over the continents inevitably under the direction of the adjoining oceanic crust. The angle of slope of the deepwater sides of the oceanic basin is generally above five degrees.*

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* The angle of slope on the sides of the Oceanic basin has not yet been properly investigated. The margin of the basin on the Atlantic border is now in about 100 fathoms water (600 feet). According to soundings by the Coast Survey, as I am informed by Mr. A. Lidenkohl of the Coast Survey Office, through J. E. Hilgard, Esq., Assistant-in-Charge, the slope between 100 and 200 fathoms off Cape Hatteras is 2° 31′; off New York entrance, 2° 02′; off George's Shoal, 1° 35′. But for the region beyond 200 fathoms, the data are not sufficient for any certain conclusion. Mr. Lidenkohl observes: "If the soundings by Lieut. Murray off Cape Lookout can be trusted, the slope between the 100 and 2000 fathom line must be over 7 degrees. Berryman's soundings off St. George's Bank indicate a slope of about 3¼ degrees. From this it may be inferred that the slope rather increases than decreases beyond the 200 fathom line."

This conclusion is further sustained by the known universality of oscillations over the oceanic basin. The central Pacific area of coral islands—"registers of subsidence"—stretches from the eastern Paumotus to the western Carolines, ninety degrees in longitude; and it indicates that the comparatively recent coral-island subsidence involved a region stretching over more than one-fourth the circumference of the globe. The fact teaches that the movements of the globe, which have been in progress through all time in obedience to the irresistible energy generated by contraction, have been world-wide, and so world-developing, even down to the latest era of geological history.

The above considerations sustain me in the opinion expressed in 1856 (this Journal, xxii, 335), that the relation in size between the mountains and the bordering oceans is not merely "formal," as pronounced by my friend Prof. LeConte, but has a *dynamical* significance.

In view of the considerations here presented, I believe there is no occasion to reject the fourth proposition (4 *a*) on page 424; but only to modify it as follows:

4*a*. Owing to the general contraction of the globe, the greater size of the oceanic than the continental areas, and the greater subsidence from continued contraction over the former than over the latter, and also to the fact that the oceanic crust had the advantage of *leverage*, or, more strictly, of obliquely upward thrust against the borders of the continents, because of its lower position, *therefore*, these borders within 300 to 1,000 miles of the coast, etc.

5. *Mountain-making slow work.*

To obtain an adequate idea of the way in which lateral pressure has worked, it is necessary to remember that mountain elevaion has taken place after immensely long periods of quiet and gentle oscillations. After the beginning of the Primordial, the first period of disturbance in North America of special note was that at the close of the Lower Silurian, in which the Green Mountains were finished; and if time from the beginning of the Silurian to the present included only fifty millions of years—which most geologists of the present day would consider much too small an estimate—the interval between the beginning of the Primordial and the uplifts and metamorphism of the Green Mountains, was at least ten millions of years. The next epoch of great disturbance in the same Appalachian region was that at the close of the Carboniferous era, in which the Alleghanies were folded up: by the above estimate of the length of time, thirty-five millions of years* after the commencement of the Silurian; so that the Appalachians were at least 35,000.000 of years in making, the preparatory subsidence having begun as early as the beginning of the Silurian. The next on the Atlantic border was that of the displacements of the Connecticut River Sandstone, and the accompanying igneous ejections, which occurred before the Cretaceous era:—at least seven millions of years, on the above estimate of the length of time, after the Appalachian revolution. Thus the lateral pressure resulting from the earth's contraction required an exceedingly long era in order to accumulate force sufficiently to produce a general yielding and plication or displacement of the beds, and start off a new range of prominent elevations over the earth's crust.

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* These estimates of the relative lengths of ages are based on the maximum thickness of their rocks—very uncertain data, but the best we have.

6. *System in the mountain-making movements on the opposite borders of the North American Continent, and over the Oceanic areas.*

A summary of the general system of movements and mountain-making on the opposite borders of the continent, and over the oceanic areas, will, I think, render it apparent that the views here sustained have a broad foundation.

I omit any special reference to the Archæan elevations, and also the local disturbances in the Primordial of Newfoundland, as well as the facts relating to minor changes of level.

A. Mountain-making on the Atlantic border.

(1.) At the close of the Lower Silurian, or a little earlier, a culmination of the great Appalachian geosynclinal resulted in displacements, plications and metamorphism, and the making of a *synclinorium*, along the *Green Mountain* region—these mountains (some summits at present over 4,000 feet high above the sea) being the result. The depth to which the region subsided during the Lower Silurian era, and the thickness of the accumulations, are not ascertained; probably the extent was not less than 20,000 feet.

(2.) Simultaneously, a *permanent anticlinorium* was made over the Cincinnati region, from Lake Erie into Tennessee, parallel with the Alleghanies of Virginia, 250 miles to the northwest.

(3.) The Acadian region—embracing western Newfoundland, St. Lawrence Bay, the Bay of Fundy, and part of Nova Scotia and New Brunswick adjoining, and probably the sea southwest between St. George's Bank and the coast of Maine, with also an area in Rhode Island—was the course of a great geosynclinal, or a series of them, parallel in general direction with that of the Appalachian region; it continued in progress, but with mountain-making interruptions, and some shift of position to the eastward, from the Silurian to the close of the Jurassic.

At the close of the Lower Silurian, no general disturbances occurred in this Acadian region, so far as is known. In the Anticosti seas, or northern part of St. Lawrence Bay, limestones, as Logan states, were uninterruptedly in formation from the beginning of the Hudson period of the Lower Silurian to the middle of the Upper Silurian, showing that the Acadian geosynclinal was then in regular progress. It so continued until—

The close of the Devonian, when disturbances, plication and metamorphism took place in eastern Canada, Nova Scotia and the bordering region of New Brunswick, and the most extensive of Acadian Paleozoic synclinoria resulted, according to the observations of Dr. Dawson and others.

(4.) The close of the Carboniferous age was an epoch of mountain-making in the Allegheny region, the Alleghanies from New York to Alabama having been then made, as already explained.

(5.) At the same time there were disturbances and synclinorian plications in the Acadian region. During the Carboniferous era, according to Logan and Dawson, 16,000 feet of rock had in some parts accumulated, and therefore a geosynclinal of 16,000 feet formed, the rocks in their many coal-beds and root-bearing layers bearing evidence, to the last, of oscillations involving an intermittent but progressing subsidence. The synclinorium, the resultant, was much less marked than that at the close of the Devonian.

(6.) During the Paleozoic, along the sea-border, a more or less perfect barrier was made by a geanticlinal uplift (anticlinorian), which was a counterpart to the geosynclinal of the Appalachian region. (See beyond.)*

(7.) The middle or close of the Jurassic period was an epoch of displacements, and the making of a series of imperfect synclinoria along the Triassico-Jurassic areas from Nova Scotia to Southern North Carolina, as sufficiently described.

(8.) During the era of the Connecticut River sandstone (Triassico-Jurassic) a nearly complete sea-border anticlinorium existed—a counterpart to the progressing geosynclinal. Its existence is proved by the absence of all marine fossils from the beds.*

(9.) The era closing the Cretaceous, and that of the Tertiary, witnessed but small uplifting and some local displacements of the rocks of these eras on the Atlantic border. The principal movement was geanticlinal, and it involved probably the whole Alleghany region.

(10.) In the Quaternary there were extended movements of geanticlinal and geosynclinal character which need not be here described.

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* In my Manual of Geology, the probable existence of such a barrier is recognized in connection with the remarks on the geography of the Trenton period in America; and it is particularly dwelt upon, and illustrated by a map, in the chapter on the Triassic; but it is not spoken of as connected in origin with the geosynclinal that was in progress to the west of it. Evidence with regard to this anticlinorium is given in the following part of this memoir.

B. Mountain-making after Archæan time on the Pacific border, within the territory of the United States.

(1.) At the close of the Lower Silurian, none yet known.

(2.) At the close of the Devonian, none yet known.

(3.) At the close of the Carboniferous age, or the Paleozoic, none yet known; and if none really occurred, then the contracting globe at that time, as far as U. S. N. America is concerned, must have expended its energies, which it had been gathering during the Paleozoic, in making the Alleghanies and in some minor plications along the Acadian region

The "Great Basin," between the Sierra Nevada on its western border, and the Wahsatch range on its eastern (lying along the meridian just east of the Great Salt Lake), contains a number of short ridges, parallel to these lofty border ranges, some of which are quite high;* and they consist, according to King, "of folds of the infra-Jurassic rocks"; and "it is common to find no rocks higher than the Carboniferous," owing, it is stated, to the erosion that has taken place. It is not clear that part, at least, of the Great Basin plications may not have taken place before the Jurassic era. If not, then the movements must have been in some way involved with those of the Sierra and Wahsatch regions.

(4.) At the close of the Jurassic, two great geosynclinals, which had been in progress through the Paleozoic and until this epoch in the Mesozoic, culminated each in the making of a lofty synclinorium—one, the Sierra Nevada, some of whose summits are over 14,000 feet high; the other the high Wahsatch, a parallel north and south range.

Whitney has proved that the Carboniferous and Jurassic rocks are comformable in the Sierra Nevada range, and that the close of the Jurassic was the epoch of its origin; but direct proof is not yet found that the Devonian and Silurian formations are included. The granite axis of the chain probably indicates, as LeConte has suggested, the region of maximum disturbance and metamorphism.

The Wahsatch contains, according to Clarence King, formations of all the ages from the Lower Silurian to the Jurassic, and the whole are throughout conformable; and a great thickness of crystalline rocks exists beneath, supposed to be Archæan, which he states are conformable also. The plications and mountain-making took place, as King states, cotemporaneously with the same in the case of the Sierra, before the Cretaceous era, the Cretaceous beds lying on the Jurassic unconformably.

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* An admirable chart, giving in detail the topography of this whole region, and including the Wahsatch, has been prepared by Mr. James T. Gardner after careful surveys by himself, topographical surveyor of the Exploration of the Fortieth Parallel under Clarence King, and is now ready for the engraver. Mr. King has published thus far only brief chapters on the geological results of his survey, in the volume of J. T. Hague on Mining Industry (vol. III). He has ready for publication Vols. I and II, on Systematic and Descriptive Geology. The Botanical Report of the Survey, Vol. V, has been issued; but Vol. IV, on Zoölogy and Paleontology, remains to be completed.

These two synclinoria are 400 miles apart. The preparatory geosynclinal of the Wahsatch—and probably that of the Sierra—took for its completion, supposing it to have begun with the opening Silurian, a period at least a fifth longer than the whole Paleozoic.

(5.) At the close of the Cretaceous, another pair of geosynclinals, parallel with the coast, but geosynclinals of only Cretaceous origin, culminated in synclinoria.

One of the Cretaceous geosynclinals was in progress *east of the Wahsatch*, along the whole summit region of the Rocky Mountains, in the United States. Directly east of the Wahsatch, according to King, the beds are 9,000 feet or more thick; and, as Hayden states, they have a great thickness in the Laramie Plains, and little less over the upper Missouri region; so that the downward movement was in some parts a profound one, and affected a very wide extent of country. Hayden and King make this disturbance to have taken place after part, or all, of the Eocene period had passed, while Prof. Marsh holds that it occurred at the close of the Cretaceous period.*

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* Clarence King has very briefly described the Wahsatch region, as well as the country to the west, in the third volume (4to, 1870) of his United States Geological Exploration of the 40th parallel; and on page 454, he says: "Subsequent to the laying down of the old Cretaceous system, and of those conformable freshwater beds which close the coal-bearing period, another era of mountain uplifts occurred, folding the coal series [Cretaceous and Lower Tertiary] into broad undulating ridges having a general trend of northeast." He then observes that freshwater Tertiary beds of sand and clay, "an immense accumulation," were laid down *unconformably* over this upturned Cretaceous, and, after the Miocene era, were subjected to "orographic" disturbance and "tilted to an angle of 15° to 20°, or thrown into broad and gentle undulations wherever they lie in the neighborhood of the older ranges such as the Wahsatch and Uintah." These disturbances were confined to within 15 miles of the Wahsatch. The period in which they occurred witnessed also great outflows of trachytic rocks in this and other parts of the Rocky Mountain region. Mr. King adds, on page 455, that there is no question as to the identity of the beds that *overlie unconformably* the Cretaceous folds along the eastern flank of the Wahsatch with the horizontal Tertiary deposits of the Green River Basin; and that over this basin between the Green River and the Wahsatch, no single instance of conformity occurs between the coal beds and the overlying horizontal freshwater strata. As stated above, he makes the epoch of Cretaceous uplifting to have followed, not the Cretaceous period, but the earliest period of the Tertiary, Eocene beds being, in his view, included with the Cretaceous in the folds referred to.

Dr. Hayden has investigated with much detail the Green River Basin and the region east of it, and years since announced that the Lower Tertiary beds, in some parts of the Rocky Mountain region, were tilted at a high angle. He has held that all the Coal-bearing strata were Lower Tertiary; but now agrees with the view expressed by King, and first suggested by Meek, that part are Cretaceous, while another part are Lower Tertiary, and considers the later Tertiary beds, which lie unconformably on the beds below in the regions of disturbance, Miocene and Pliocene. He states that the thickness of the Cretaceous formation in the Laramie Plains is 8,000 to 10,000 feet. He observes in a recent letter to the writer that the Coal-bearing strata and Cretaceous are never unconformable; but

The other geosynclinal belt of the Cretaceous era was to the *west of the Sierra Nevada*, as described by Whitney. This coast geosynclinal ended in extensive displacements and plications, much metamorphism, and a high synclinorium.

(6.) The intermediate region—the Great Basin, which had been widened at the close of the Jurassic by the annexation of the plicated and consolidated Sierra and Wahsatch—was the area of a geanticlinal, or at least of absence of subsidence; for King says no Cretaceous rocks occur over it.

(7). With the close of the Cretaceous, or when the Cretaceous synclinorian movements of the sea-coast and mountains were ending, a geanticlinal movement of the whole Rocky Mountain region began, which put it above the sea-level, where it has since remained. This upward movement continued through the Tertiary.

(8.) During the Tertiary age, until the close, probably, of the Miocene Tertiary, another pair of parallel geosynclinals—but geosynclinals of Tertiary formation—were in progress. The Cretaceous synclinoria had given still greater breadth and stability to the relatively stable region between them, and one of these new troughs is hence farther east on the mountain side, and the other farther west on the coast side.

In the coast geosynclinal, marine Tertiary beds were accumulated to a thickness of 4000 to 5000 feet; and then followed the epoch of disturbance ending in another coast synclinorium, a coast range of mountains, in some places metamorphic, and having ridges, many of which are at present 2,000 feet or more in height above the sea, and some in the Santa Cruz Range, according to Whitney, over 3,500 feet.

The other is to the east of the Cretaceous axis in the summit region of the Rocky Mountain chain. A great thickness of freshwater beds was made in the Green River region and some other places about the Rocky Mountain summits, and thinner deposits to the eastward. The thickness, in connection with

instead are often folded together, and sometimes stand at a high angle, even vertical in many places, as in the Laramie Plains south of Fort Sanders; along the Big Horn region; between Long's Peak and Pike's Peak; near Denver in Colorado, etc. Near the mouth of the Big Horn. The Chetish or Wolf Mts. consist of these upturned strata and have a height of 1500 to 2000 feet above the Yellowstone. He found the later Tertiary beds sometimes tilted at a small angle, never over 10°.

The discovery of Dinosaurian remains in some of the Coal beds, announced by Marsh and Cope, and of *Inocerami*, as ascertained by Meek, is one part of the evidence on which the lower parts of the Coal beds is determined to be Cretaceous. Besides this, there is the fact that the supposed Miocene of the Green River Basin contains remains of mammals that are decidedly Eocene in character; and if these are Eocene, then the Coal beds are something older. Prof. Marsh is very strongly of the opinion that all the *Coal beds are Cretaceous.*

On the other side, Lesquereux states that the evidence from fossil plants is totally opposed to making any of the Coal strata Cretaceous.

The method of mountain-making, and the principle involved, are the same whatever be the decision as to the exact epoch of the Cretaceous plication.

evidences of shallow water origin, indicates a progressing geosynclinal, although the ocean gained no entrance to it. The downward bending ended probably just after the Miocene period without general displacements; but there were tiltings along the more western border of the Tertiary in the vicinity of the Wahsatch and other mountains. (See note on page 439.)

(9.) Since the Miocene era, and on through much of the Quaternary, there have been vast fissure-eruptions over the western Rocky Mountain slopes. They had great extent especially in the Snake River region where the successive outflows made a stratum 700 to 1000 feet thick, over an area 300 miles in breadth. There are other similar regions but of less area.

It is thus seen that along the Pacific side of the continent the crust, under the action of lateral pressure, first bent downward profoundly, and then yielded and suffered fracture and plications, directly along a belt, parallel with the coast, either side of the Great Basin (and perhaps over this basin to some extent), the two great lines 400 miles apart. The plicated regions, thus made, having become firm by the continued pressure and the engendered heat and resultant solidification, the crust next bent, and then yielded, in a similar way, along an axis outside of the former regions of disturbance, the two axes over 600 miles apart; and again all was mended in the same way. Then it bent a third time, just outside of the last range, on each side of the same great area, the lines over 700 miles apart; and then, over the western of the two ranges, the beds were displaced, solidified, and left in high ridges; but over the eastern the final disturbances were local and slight.

There were hence two parallel series, cotemporaneous in steps of progress, situated on opposite borders of the Great Basin, a *coast* series, and a *mountain* series, each having its highest member toward the basin; the coast series, the grandest in its three parts, and leaving evidences of the profoundest disturbance, and the greatest amount of metamorphism. The Wahsatch range is nearly as high as the Sierra; but probably a fourth of its height is due to the final elevation of the Rocky Mountain region.

The last bendings were more local than the preceding because the crust had become stiffened by its plicated and solidified, and partly crystallized, coatings, as well as by thickening beneath; and therefore, while the Tertiary movements were in progress, the part of the force not expended in producing them carried forward an upward bend, or geanticlinal, of the vast Rocky Mountain region as a whole. For the same reason, profound *breakings* took place where bending was not possible, and thereby immense floods of liquid rock were poured out over the surface. (Most of the great mountains of the globe were lifted about this time, that is, in the course of the Tertiary era, and many of the great volcanoes were made.)

There were irregularities or exceptional courses in connection with this system of movements and their effects. But these show only that in the same area the lateral pressure at work was not alike either in amount, or in direction, in different latitudes; nor was the resistance before it the same.

The results correspond with the well-understood effects of lateral pressure. Suppose a long beam, having an even texture except that a portion toward the middle (say a sixth of the whole length) is stouter than the rest, to be subjected at its extremities to direct pressure. The first yielding and fracture would take place toward the stouter portion on either side. If this break were mended by splicing and cementing until firmer than before, the next region of yielding would be just outside of the former. In brief, the fracturing would be in each case near the stouter portion of the beam. Moreover, the extent of the yielding and fracture on each side would have some relation to the amount of pressure against that side. Just so has it been with the earth's crust under the action of lateral pressure. The facts further illustrate the truth, before announced, that the force from the ocean side had in some way the advantage, and in fact was the greater. But the full difference is not indicated by the difference in the results of disturbance, since the shoving force on the side of greatest pressure would not be limited in its action to its own side, unless the intermediate stouter region were wholly immovable.

C. Movements over the Oceanic areas.

The history of the changes of level over the oceanic areas is necessarily a meager branch of geological science. There are, however, some great truths to be gathered which are of profounder import than is generally acknowledged. They show that the occeanic crust has sometimes acted in the capacity of a single area of depression, although of so immense extent. I allude briefly here to only two of the facts, referring the reader to my former articles for a fuller discussion of the subject.

First, the remarkable one that nearly all the ranges of islands over the Pacific ocean, and even the longer diameters of the particular islands, lie nearly parallel with the great mountain ranges of the Pacific coast of North America. There is a dynamical announcement in this arrangement—which is partly recognized when we refer it, as I have proposed, to the existence of directions of easiest fracture in the very nature of the infra-Archæan crust, and regard the courses of these feature lines of the oceans and continents as having reference to one of these directions. But, besides this, there is a declaration with regard to the *direction* of the pressure that acted against the continents and reacted over the oceanic areas.

The other fact is that of the Coral island subsidence, already referred to, which affected the tropical ocean for its whole breadth, or more than a quarter of the circumference of the globe; sinking the sea bottom at least 3000 feet over a large part of the area, and probably over its axial portions two or three times this amount.* The oceanic basin was evidently one basin in its movement; but the areas of less and greater subsidence, of parallel N.W. by W. trend, so alternate along the southern border of the region of subsidence that we may conclude there were great parallel waves, made by lateral pressure in the crust, as I have elsewhere explained,† that is, geosynclinals and geanticlinals, such as are the only possible conditions of the crust under the lateral pressure of contraction. Now this great oceanic subsidence, involving the breadth of the ocean, if begun in the Tertiary era, as is probable, was going forward at the very time when the Rocky Mountains, and other great mountains of the globe, were in progress of elevation, as if these were counterpart movements in the earth's surface; and it continued on during the Glacial era, when the continental elevations appear to have reached their highest limit.

We gather from these facts how it is that a general submergence, or an emergence, might characterize cotemporaneously large areas of North America and Europe; as, for example, in the Subcarboniferous, Carboniferous and Permian periods, during which the rocks show that there was a general parallelism in the movements. If a geanticlinal were in progress over the middle of the Atlantic crust, as a result of the lateral thrust in the continental and oceanic crusts, there might also be a reverse movement or general sinking along the continental borders, as well as a rise of water about the continents from the diminution in the ocean's depth; and when the oceanic geanticlinal flattened out again through subsidence, the subsiding crust would naturally produce a reverse movement along one or both continental borders.

From the various considerations here presented, derived from both the continental and oceanic areas, it is apparent that the earth has exhibited its oneness of individuality in nothing more fundamentally and completely than in the heavings of its contracting crust.

The subjects of metamorphism, the earth's interior, igneous eruptions and volcanoes remain for discussion. In addition I propose to consider the steps in the origination of the continental plateaus and oceanic basins, and also present some facts bearing on the general nature of the infra-Archæan crust, that is, the part below the earth's superficial coatings.

* Author's Rep. Geol. Wilkes U. S. Expl. Exped., 4to, 1849, p. 399; and Corals and Coral Islands, 8vo, 1872, p. 329.

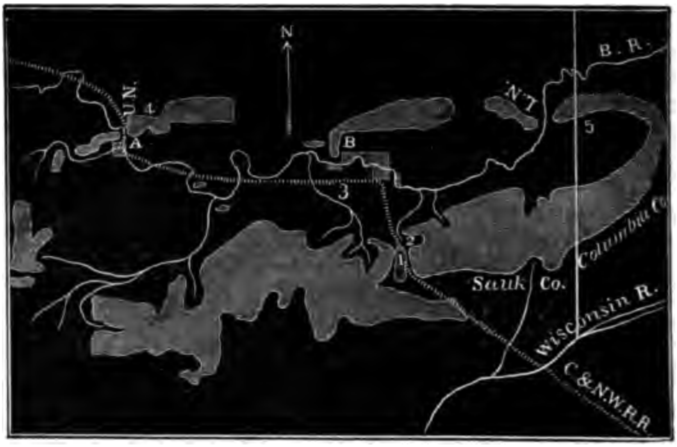
† Rep. Geol. Expl. Exped., p. 399; Corals and Coral Islands, p. 328.

Art. XLVII.—*On the relations of the Sandstone, Conglomerates and Limestone of Sauk County, Wisconsin to each other and to the Azoic;* by Prof. James H. Eaton.

The age of the quartzite hills and ridges of Sauk County has been satisfactorily determined by Mr. Roland Irving* to be Pre-silurian. Mr. James Hall† in his report of the State Survey calls them Huronian. On Dr. Lapham's map a small region on the Eau Claire River, adjacent to the great central area of granitic rocks, is colored as quartzite. An examination of this locality, to determine whether the latter rest unconformably upon the former, would perhaps determine their age. For the present we can say that these rocks differ lithologically.

The accompanying map is by Mr. Wm. H. Canfield, of Baraboo, who for many years has been the officialy surveor for Sauk County, and it is taken from surveys made by him with the especial purpose of marking the quartzite outcrop. It has been completed for Columbia County by Mr. T. C. Chamberlain, of Whitewater. The dotted line east of the Upper Narrows was also added by Mr. Chamberlain. It is believed that this map shows the entire outcrop of Azoic rocks in the region of the Baraboo River.

1.



A, Abelman; B, Baraboo; B R, Baraboo River; L N, Lower Narrows; U N, Upper Narrows. 1, Devil's Lake; 2, 3, Potsdam Sandstone; 4, Section; 5, Limestone. Scale, three-twentieths of an inch equal to a mile.

We have thus represented a group of islands which existed in the Potsdam sea, with their common trend east and west, or at right angles to the dip of the rocks.

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* This Journ., Feb., 1872.

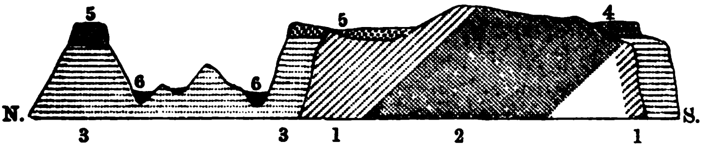
† Survey of Wis., p. 11.

The following localities were visited by myself in the fall of 1872:

The point marked (2) on the map is the locality from which the fossils were obtained, which were described by Mr. Alexander Winchell* in 1864. In a short time a large number of fossils were obtained from loose pieces of sandstone. I was assured that there is a quarry of the rock in a place near by, but the time did not suffice to find it. The fossils were *Scolithus linearis* Hall, *Orthis Barabuensis* Winchell, *Straparollus primordialis* Winchell, *Dicellocephalus Minnesotensis* Owen.

At the locality marked (4), however, is a section (fig. 2) which is truly magnificent in its exposure, of all the rocks which belong to this region, in their stratigraphical relations, except the limestone. It is at the railroad station, Abelman. The Baraboo River, in forming the Upper Narrows, has left upon the east side a nearly vertical section, about half a mile long and 300 hundred feet high at the highest point. This section is of a core of tilted rock, flanked on *both* sides by horizontal Potsdam sandstone and conglomerates. No doubt can therefore remain that the tilted rock is Pre-silurian.

2.—SECTION AT UPPER NARROWS, BARABOO RIVER.



1, Quartzite. 2, Metamorphic Conglomerate, 3, Potsdam Sandstone- 4, 5, Conglomerates. 6, Drift.

The dip of the entire section of Azoic rock is to the north or slightly west of north. Its face is cut by numerous vertical joints in the same manner as the cliffs at Devil's Lake. At the extreme southern end the rock varies from a compact dark-colored homogeneous quartzite, to a much less compact and lighter quartzite. One large detached block of the hard, dark quartzite was seen beautifully covered with ripple-marks.

Passing along the face of the cliff toward the north it becomes covered with large blocks of quartzite, sandstone and conglomerate, which have fallen from above.

Coming to the exposed rock again, it changes to a metamorphic conglomerate. This makes up more than half the section. It consists of *angular* pieces of the compact Sauk quartzite, firmly imbedded in a cement of white crystalline quartz. The former vary in size from small fragments to

* This Journ. II, vol. xxxvii, p. 226.

masses several tons in weight. Numerous cavities are lined with quartz crystals. The dip here is from 75–80° N.

The remainder of the section consists of the homogeneous, dark, compact quartzite, bedded in the same manner. We have then indications of three successive sets of circumstances in Azoic times; those in which were formed respectively the underlying quartzite, the conglomerate and the overlying quartzite. The lower quartzite must have been already hardened from the moving sands before it was broken into fragments for the conglomerate. And then its cement was crystallized. Finally, layers of sand spread over this were hardened.

As has been said, upon the southern flank of this Azoic core, horizontal beds of Potsdam sandstone lie unconformably. They extend a short distance over the edges of the upturned beds of quartzite.

The relation of this sandstone to the underlying quartzite shows most unmistakably the effects of shore action. The quartzite is generally in place, but the large blocks, formed by the crossing of the planes of bedding and the joints, are somewhat isolated, as if they had formed crags on an old coast, where the wearing of the waves had enlarged the cracks. Into these fissures and crevices the sand is forced. There are also blocks of quartzite, that have been displaced somewhat, which are enveloped in sandstone. In the sandstone itself is an occasional rounded pebble of quartzite. The sandstone which rests upon the northern flank is irregularly bedded, having the ebb and flow structure. Farther north are three isolated hills of sandstone.

Resting on the sandstone at the south, and stretching also over the quartzite, is a conglomerate made up of a friable sandstone, like that below, containing numerous rounded pebbles of the quartzite of various sizes. The cement makes up a considerable part of the rock. This conglomerate, as I have assured myself by careful examination, is exactly like that mentioned by Mr. Irving as occurring on the quartzite just northeast of Devil's Lake, and containing Potsdam fossils. The finding of this conglomerate, therefore, in its true relation, verifies Mr. Irving's supposition in opposition to Mr. Winchell, that neither the conglomerate nor the quartzite is the *base* of the Potsdam system, for here the true base comes in between, as sandstone. In the same manner there is a conglomerate at the north resting conformably on the sandstone and unconformably on the quartzite. One of the isolated hills of sandstone is also capped by the same. As nearly as could be determined, the level is the same as that of the conglomerate at the south. But its character is different. It is made up almost entirely of small rounded pebbles of quartzite of a pretty uniform size. The cement is quite hard, but true sandstone.

This section then represents an old Azoic reef of tilted rock, running east and west, washed upon either side by the waves of the Potsdam sea. On the south the action appears to have been gentler than on the north, for while at the south the quartzite has been triturated to a fine sand, containing, to be sure, larger or smaller pieces of quartzite, well rounded, the northern shore must have been exposed to the breakers which washed out the fine sand and left pebbles of a uniform size. It may be that within the circle of these islands was a sheltered bay. Mr. Chamberlain has observed at a little distance back from the edge of the cliff, sandstone again covering the conglomerate and, in fact, the entire length of the quartzite, indicating a subsequent subsidence of the entire reef below the water.

At the point marked (5) on the map is a limestone quarry. It is horizontally bedded. All points of junction with the underlying rock are concealed, but it is plainly, at least, 100 feet below the Potsdam sandstone in place. Whether it is a local deposit in the Potsdam sandstone or is the Lower Magnesian limestone, I have not yet determined. The latter supposition requires an enormous erosion between the putting down of the Potsdam sandstone and the Lower Magnesian limestone. A number of fossils were secured, several cephalic shields of a trilobite, a *Pleurotomaria?* and others still more indefinitely known.

Another feature of interest in this region is the evidence of glacial action aside from the drift. At the point (3) on the map is an isolated hill of sandstone. On my visit the earth had just been removed from a large surface in order to quarry the rock. It was entirely smoothed and covered with glacial striæ. Their direction was N. 66° E. On the surface of the limestone previously mentioned, the polishing is even more perfect, and the striæ have the same direction. The only way I can explain this deflection from the usual direction is, that it was caused by the trend of the ridges. At the Glacial epoch, the erosion of the Baraboo Valley must have been as great as at present.

Mr. Canfield writes me: "The polished surface of quartzite that I especially mentioned to you, * * * showing probable glacial rubbing, was three-fourths of the way up the bluff in section 27. I also stated to you my convictions that the evidences of that character could be found all over the tops of the bluffs, within the glacial limit, which is east of the 'Lake of the Bluffs.' There is certainly a great deal of granitic debris piled upon the top of the bluffs."

Beloit, Wisconsin, Feb. 14, 1873.

ART. XLVIII.—*On the formation of the features of the Earth-surface. Reply to criticisms of T. Sterry Hunt;* by JOSEPH LECONTE, Prof. Geol. Univ. of California.

IN the April number of this Journal, p. 264, Prof. Hunt reviews my paper "On the formation of the great features of the earth-surface,"* criticising *some* points and making reclamation of *others* for himself. In his criticisms he has sometimes misunderstood and sometimes, I think, hardly fairly represented me. In his reclamations, it seems to me, that, in his anxiety to press *yet once more* upon the attention of geologists his own labors, he has mistaken the *use of similar* materials for the *similar use* of materials. That I have used materials similar to those used by himself and many others, I admit; but I have used them certainly in a different way and for a different purpose. Before I commence to show this, however, let me at once acknowledge my deep indebtedness to Mr. Hunt for the many valuable suggestions which I have gotten from the thoughtful and repeated study of his numerous papers. I frankly confess that it was the study of these that first turned my attention to what I now regard as the true *starting point*, and for this I most heartily thank him; but from this starting point we have certainly worked in somewhat different directions, as I now proceed to show.

1. My starting point is the proposition that "the whole foundation of theoretic geology must be reconstructed on the basis of a solid earth." But Mr. Hunt states that this has been his starting point ever since 1858. This is very true. I did not say it was a *new* starting point. I am sure no reader of my paper could imagine that I made any such claim. On the contrary, this starting point is necessitated by a belief in the solidity of the earth, to which belief, I had just previously stated (p. 352), many of the most advanced geologists were fast coming. Among these advanced geologists, I had in my mind Mr. Hunt, but did not think it necessary to mention so obvious a fact. I am sorry, now, I did not If I deserve any credit at all in connection with this much coveted starting point, it is in collecting and arranging the evidence against the *other* starting point, and in favor of this one, and thus justifying my selection as the only rational one.

2. Again: after leaving the starting point, one of the important steps in my march was *the aqueo-igneous fusion of deeply buried sediments and their consequent metamorphism by the rise of the geo-isotherms.* But Mr. Hunt claims that this, too, he has

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* This Journal, vol. iv, p. 345 and p. 460.

been insisting upon for years. Very true, again. I suppose there is no geologist in this country, or in Europe, who is not fully aware of this. Mr. Hunt does not give sufficient credit to the good memory of geologists, or to the importance of his own papers, or he would not have thought it necessary to mention it again. If the idea had been original with Mr. Hunt, and confined to him, it would have been necessary to credit it to him; but it had already become the common property of science; it had been used by Herschel, and Scrope, and Lyell, as well as by Hunt and Hall. Surely, I did not claim originality, for I spoke of it as a well known *recent* view (p. 467); my only fault, then, was not mentioning Mr. Hunt. I am sorry, again, I did not do so, but I can assure him it was only because I regarded his writings as so well known.

3. Again, Mr. Hunt had stated in one of his early papers that the aqueo-igneous fusion of deeply buried sediments "offers a ready explanation of the phenomena of volcanoes and igneous rocks," and he now states that my views are similar to his. Mr. Hunt, it seems to me, has speculated somewhat vaguely on this subject. Sometimes he speaks of a *zone* or *region* of plastic matter separating a solid nucleus from a solid crust (p. 264), through which zone the shrinking nucleus would act on the solid crust precisely as if the whole interior were liquid; sometimes he speaks more truly of *local* masses of plastic matter beneath great *recent* accumulations of sediments. The generation of gases and vapors within the sediments is, according to him, the *force* of eruption. Now, according to my view, a zone of liquid matter would not do at all: the liquid mass is local, and is *squeezed out* by horizontal pressure through fissures, as great *massive eruptions*, often forming the great mass of mountain chains, and must be *local* in order that it should be squeezed out. Volcanoes, I suppose, are parasites on these great out-squeezed masses, produced by the access of water to their still hot interior. Let it be remembered, however, that my subject was a theory of *mountain chains;* my principal object, therefore, was not a general theory of volcanoes, but to explain the *association of both massive eruptions and volcanoes with mountain chains.*

4. I had stated (p. 468) that the aqueo-igneous fusion of sediments determined lines of weakness and, therefore, lines of yielding to horizontal pressure. Mr. Hunt now states that he had previously expressed similar views in similar language. I freely admit this, although I did not know it when I wrote; it had escaped my memory. This is the only sin I have to acknowledge. As far as I know, the idea is original with Mr. Hunt, and I ought to have known it and credited him with it. I now do so most heartily. But observe, my main object was

not the fact itself, but the *use* of the fact in sustaining my theory. All I claim here, therefore, is the connection of this fact with the *position and formation of mountain chains.*

5. I attribute the enormous *foldings* of the strata of mountain chains to horizontal crushing together, produced by the interior contraction of the earth. Mr. Hunt makes reclamation of this also. Let us compare our views on this subject. Mr. Hunt attributes the folding of the Appalachian chain to three causes; (*a*) Subsidence of a convex mass of sediments; (this I have shown (p. 461) could not take place if the sedimentation and the subsidence went on *pari passu.* (*b*) Contraction of the strata by metamorphism; (this I suppose could only produce foldings by producing subsidence of the convex surface, and therefore, is subject to the same difficulties as the last.) (*c*) Horizontal pressure produced by interior contraction. The first, Mr. Hunt says, is probably not the *principal* cause, but I cannot think he regarded the last as a great cause, for *he does not connect it with elevation of the chain.* Now, according to my view, *folding is produced entirely by the horizontal crushing; and indissolubly connected with this crushing together horizontally, is the up-swelling vertically, and the formation of the mountain chain.* Again, Mr. Hunt's Appalachian foldings were going on during the whole process of sedimentation; mine commenced only after the sedimentation was completed. That the latter view is the true one, is proved by the fact that the last layer of the Coal-measures is not only folded, but folded equally with the lower strata. That the folding took place only after sedimentation was completed, is not, of course, original with me: it is the common belief of geologists. All I claim is the *connection of foldings, on the one hand, with horizontal crushing as a sole cause, and on the other with the formation of the chain.*

6. I stated (p. 462), that Hunt and Hall leave the sediments just after the whole preparation had been made, but before the actual mountain formation has taken place, and, therefore, this theory had been characterized as a theory of mountains with the mountains left out. Mr. Hunt, referring to this statement, says that Hall and himself had been much misunderstood by myself as well as by others; that neither he nor Hall ever proposed any theory of mountain formations at all, but only a return to the views of Buffon and Montlosier, that "mountains are fragments of denuded continents."

In order to make my explanation of this point clear, I find it necessary to define my terms. The word *mountain* is *loosely* used, in scientific as well as in popular language, to express every considerable inequality of the earth surface, from a great mountain chain like the Andes or the Himalayas, to mere hills of circumdenudation like those on the upper Mississippi. The result has been much confusion of thought. For it is evident that the great bulge which constitutes a mountain chain, and which can be seen only from a distance, is formed in an entirely different way from the smaller inequalities which constitute scenery; the former is evidently produced by general causes affecting the whole earth, the latter wholly by *erosion.* I think therefore, that it is necessary very carefully to distinguish these in our theories. In my own lectures I no longer divide mountains into two kinds, mountains of *upheaval* and mountains of *erosion*, but simply treat the whole subject of mountains under the two heads of *mountain formation* and *mountain sculpture.* All portions of continents, it is true, are sculptured in this way, but this is especially true of mountain chains, which are the great theaters of erosion as of igneous agencies. When I speak of mountain formation, therefore, I mean only the formation of the great bulge or convex plateau which constitutes the *chain;* but when Mr. Hunt speaks of mountains as "fragments of denuded continents," he refers, of course, not to the chain, but to the smaller inequalities, or the effects of *sculpture.* It is certainly one of the great glories of American geology, to have *clearly* shown by the study of the Appalachian chain the immensity of this work of erosion; that not only the smaller ridges and ravines, but great cañons, wide valleys and lofty peaks owe their origin to this cause alone. To Lesley, Hall and Hunt is chiefly due the credit of expounding these views. I confess their writings have been of immense service to me in my mountain studies. But I insist that a theory of these is not a theory of *mountain chains.* The older geologists, it is true, neglected far too much the effects of erosion, and attributed every peak, and ridge, and valley, to upheaval, or fracture, or engulfment; but there still remains the great bulge or convex plateau, the real chain, to be accounted for; for no one imagines *this* to be the result of erosion.

Now it is precisely this convex plateau which, I had supposed, Hall and Hunt attributed to *sedimentation.* I had supposed that they regarded the Appalachian chain as first a great *convex submarine plateau* produced by a line of sedimentation; then this raised into a *convex mountain plateau* by continental elevation; and finally this plateau sculptured into its present forms. I admit that all of this is not clearly and definitely expressed, but when I attempted to formulate their views clearly I could arrive at no other result. Others, I believe, have arrived at a similar interpretation of their views. Under the impression that this was their view, I proceeded to show that it certainly was not true, since the Appalachian region at the end of the Paleozoic era, and immediately before the formation of the chain, was not a *convex* plateau of any kind, but a *concave* trough, and the whole bulging took place afterward by the crushing together of its strata. But now (if I understand him aright, for he is still not very clear) Mr. Hunt says that the Appalachian plateau or chain was formed by the same unknown process by which the continent was elevated; that it was formed by continental elevation, which from some unknown cause *was greater in the Appalachian region.* I wish much he had clearly expressed this at first; it would have saved much useless discussion. I confess, however, I can not find anything like this in his previous papers or in the writings of Prof. Hall. In the early presentation of a difficult subject, however, some want of clearness is pardonable.

7. According to my view, *foldings* are a necessary concomitant of mountain formation; but Mr. Hunt, p. 267, thinks both cleavage and foldings are mere *accidents*, unnecessary to mountain structure; and he cites examples of mountains on the upper Mississippi composed of *perfectly horizontal strata*, and of Catskill mountain composed of *nearly* horizontal strata, *uncomplicated with foldings.* I could add other examples from my own observations on the Sierra chain. Mt. Dana, a magnificent peak more than 13,300 feet high, on the very crest of the Sierras, is composed of strata which seem to be perfectly horizontal. But this is no objection to my theory; it is only an example of the confusion of thought of which I speak above. The explanation of the difference between mountain *formation* and mountain *sculpture*, between mountain chains and so-called erosion mountains, completely answers this objection. I was speaking of *chains*, not of isolated *peaks.* Mountain chains are, I believe, always folded; but in the extensive erosion of the *wide* folds of thick strata it would be strange indeed if *no* portions where the strata were horizontal were left as peaks. This is the case, doubtless, with Catskill and Mt. Dana. As for the erosion hills of the upper Mississippi, the explanation is still simpler. There is no mountain chain there at all; these hills are only fragments of denuded continental strata.

8. I hold the view (p. 464) that during the Paleozoic era there existed a continental mass, probably of considerable extent, to the eastward of the Appalachian region. Mr. Hunt again makes reclamation for himself and others. Surely there was no necessity for reclamation. If I had imagined that this was new, I would have devoted a whole paper to it, not have dismissed it in a single very short paragraph. But I certainly do not leave this point doubtful; on the contrary, I say "evidences are daily accumulating" on this point; not by *my* labors; for I was on the Pacific coast; but by the labors of others. I of course referred to the very evidence which Mr. Hunt mentions, but did not think it necessary to mention names in connection with facts so

well known. If I deserve any credit in this connection, it is in giving *something more of definiteness to the conception* and *especially in showing its connection with the formation of the Appalachian chain*.

9. In the last paragraph of my paper, I acknowledge my inability to explain those "great and wide-spread *oscillations* which have marked the great divisions of *time*, and have left their impress in the general unconformability of the strata; the last being that of the Post-tertiary period." After quoting this, Mr. Hunt goes on to say that it is precisely this *upward* movement which *constitutes the continental elevation of* Montlosier, Hall and himself, and *which give rise to plateaus* by the erosion of which are formed mountains. And since I regard plateau-formation the only true mountain formation, by my own admission (so argues Mr. Hunt) mountain formation is still unexplained. Thus Mr. Hunt makes me, after writing a paper to explain the formation of mountain chains, in the last paragraph acknowledge that it is inexplicable. I wonder Mr. Hunt did not rather suspect that he had entirely misunderstood me.

It was not *upward* movement or *downward* movement which I regarded as inexplicable, but *oscillation* or movement *upward and downward alternately in the same place*. All the causes of movement of which I had previously spoken would *continue to act in the same direction*, and therefore the continents ought to grow higher and larger and the sea-bottoms deeper. On the whole, this has probably been the case throughout the geological history of the earth, as has been so beautifully shown by Dana for the North American continent. The recent observations on the wonderful *persistence of deep-sea faunæ and therefore of deep-sea conditions*, through many geological periods, go far to confirm this view for the sea-bottoms. Previously geologists had mostly regarded the earth's crust only as *oscillating*; Dana showed *gradual development* or evolution of continents in the midst of oscillation. Now I have attempted to explain the *development*, but acknowledge my inability yet to explain the *oscillations*.

10. Finally, Mr. Hunt criticises, and perhaps justly, my views as to the chemical cause of the intense heat so often found in lavas. Since the publication of Mr. Mallet's paper, I much prefer adopting his views on this subject. I now, therefore, regard the process of mountain formation to be briefly as follows: lines of thick sediments, rise of geo-isotherms and aqueo-igneous softening determine lines of yielding; then crushing together *horizontally* and swelling up *vertically* forms the chain; but once the yielding commences, then *mechanical energy is changed into heat*, which may thus be increased to any amount and produce true igneous fusion.

Art. XLIX.—*Notes of Observations on Jupiter and its Satellites;* by Prof. M. Mitchell, of Vassar College. No. 2.

The following observations were made with the equatorial telescope of the observatory of Vassar College, the object-glass of which is 12⅛ inches in diameter; the powers used varied from 200 to 250. Longitude of obs. $4^h\ 55^m\ 37^s$; lat. 41° 40′ 50″.

1872, Jan. 5. At $11^h\ 12^m$ P. M. the 3d satellite and its shadow, and the 1st satellite and its shadow, were seen on the disc of Jupiter at the same time. The 3d satellite was not round, but elongated in the direction of the belts of Jupiter, as small as the shadow of the 1st and nearly as dark, the color being brown. The 1st satellite was seen as a brilliant white spot, thrown upon the dark equatorial belt of Jupiter, yet it could be seen for only 25 minutes. The disappearance of the first satellite in transit does not seem to be a consequence of difference of brilliancy of color between it and the planet.

1872, Jan. 17. $8^h\ 15^m$ to $9^h\ 12^m$. A circular white spot was seen on the lower part of the broad belt of Jupiter, sufficiently defined to be measured.

On the 25th, at 8 P. M., a spot, apparently exactly like that seen on the 17th, was observed in the upper part of the broad belt, measuring nearly the same in diameter.

1872, Jan. 30. The 1st satellite which was known to be upon the planet could not be seen until it touched the limit at its egress, although its path must have lain wholly within the dark belt. It was by measurement smaller in diameter than its shadow.

1872, Feb. 2. The 2d satellite was seen as an irregular white spot when a little past the center of the disc. The shadow appeared to be larger than the satellite, but, on measuring, was found to be smaller.

1872, Feb. 7. The 3d satellite showed a very well defined disc with no spot. Its diameter was 2″·09.

1872, Feb. 26. The 4th satellite was occulted. It became very indistinct as it approached Jupiter. Its light was whiter than that of the whitest portion of the planet. It became invisible at $9^h\ 48^m\ 42^s{\cdot}64$.

1872, Feb. 28. The night was remarkably good. Two large white spots were seen on the equatorial belt at 7 P. M. They were well defined and were measured. They were visible for a short time only, and could not be seen to follow with the planet as it turned. Dark spots seemed to succeed them in the same position on the disc.

The 3d satellite was first seen to emerge from shadow at $7^h\ 5^m\ 43^s{\cdot}4$. It was fully out at $7^h\ 11^m\ 4^s{\cdot}4$. The occultation of the 1st satellite occurred at $9^h\ 6^m\ 39^s{\cdot}4$.

1872, March 1. The 1st satellite was seen to come out of the shadow at $6^h\ 49^m\ 9^s{\cdot}9$.

1872, March 7. The 1st satellite seemed to touch the limb, in transit at $7^h\ 13^m\ 51^s{\cdot}6$, was wholly on the disc at $7^h\ 19^m\ 0^s{\cdot}6$; after which it was seen for only ten minutes.

1872, March 16. Three dark spots were seen upon the principal belt, larger and as dark as the shadow of the 1st satellite, which was also on the planet's surface. The shadow seemed to become smaller and more distinct as it approached the limb; it was last seen at $7^h\ 56^m\ 36^s{\cdot}88$. By measurement the diameter of the shadow of the satellite was larger than that of the satellite. The 3d satellite was free from spots.

1873, Jan. 19. Observations on Jupiter began at $8^h\ 30^m$ P. M. The 3d satellite was known to be in transit, but could not be seen until it had passed the center of its path. It was then an irregular dark spot. It became more round and well defined and again indistinct, although there were no perceptible changes of light and shade on the disc of the planet, and the air was steadily improving.

1873, Feb. 4. At a little after 9 P. M. the 4th satellite was seen on the disc of Jupiter as a brownish-gray marking, not far from the preceding limb. It was lost for a time, but reappeared when near the limb. Like the other satellites in that position, it showed a disc similar to that of the moon seen through mist. It was first seen to protrude beyond limb at $9^h\ 35^m\ 45^s{\cdot}20$; was wholly off at $9^h\ 38^m\ 31^s{\cdot}20$. The 3d satellite, shining far from Jupiter, showed a disc irregular in shape and hazy in outline. The broad belt of Jupiter was slightly reddish.

1873, Feb. 17. Observations began at $7^h\ 32^m$. The shadow of the 1st satellite could be seen, thrown upon the planet, it was not round, but elongated in a direction perpendicular to the broad belt, upon which it was seen. As usually happens, the satellite was seen round and snowy white a few minutes before it left the disc; it was much whiter than Jupiter. 1st satellite was wholly off at $8^h\ 47^m\ 42^s{\cdot}5$; shadow last seen $8^h\ 52^m\ 00^s$.

1873, Feb. 25. The 2d satellite was occulted at $9^h\ 49^m\ 52^s{\cdot}6$.

1873, March 11. Jupiter was seen between flying clouds, but the seeing was excellent. A faint rosy tinge could be seen on the upper part of the broad equatorial belt, on which there was a large white spot. The 3d and 4th satellites showed distinct discs; that of the 3d was ruddy in color. The 1st satellite was occulted. It touched the limb at $8^h\ 46^m\ 23^s{\cdot}1$; was bisected by the limb at $8^h\ 48^m\ 17^s{\cdot}1$; was last seen at $8^h\ 51^m\ 23^s{\cdot}6$.

1873, March 13. Seeing excellent. Four lines in the broad belt were strongly marked, but no rosy tinge could be perceived. The 2d satellite touched the limb at $8^h\ 30^m\ 12^s{\cdot}75$, was wholly within the limb at $8^h\ 38^m\ 00^s{\cdot}25$, after which it could be seen for a very few minutes.

1873, March 17. 10 P. M. The 3d satellite was seen as a dull irregular shading upon the disc of the planet. It became a well defined brownish-gray spot as it neared the center, seeming to be preceded by a minute grayish spot, possibly denoting an irregularity in the shape of the satellite. Diameter of 3d satellite $= 1''{\cdot}8$ as measured while in transit.

1873, March 28. The equatorial belt was marked by two large white spots, which seemed to narrow and elongate in an equatorial direction as the planet turned. The shadow of the 1st satellite passed from the disc at $7^h\ 22^m\ 44^s{\cdot}5$.

ART. L.—*Some remarks on the Geological Structure of a district of country lying to the north of the Grand Cañon of the Colorado;* by J. W. POWELL.

THE Colorado River of the West is formed by the junction of the Grand and Green; from this point the course of the river is a little west of south until the mouth of the Little Colorado is reached, and from this last mentioned point, its general course is to the west to the mouth of the Rio Virgen, where it turns again to the south.

The Grand Cañon extends from the mouth of the Little Colorado to the foot of the Grand Wash, a narrow, abrupt, cañon valley extending back from the Colorado River from a point about 60 miles above the mouth of the Rio Virgen. This profound gorge is more than 200 miles in length, and varies from 4,000 to nearly 6,000 feet in depth.

I propose, in this article, to discuss briefly some of the principal topographical and geological features of a district of country lying to the north of the Grand Cañon and south of the sources of the Sevier, east of the Colorado River, and west of the Grand Wash and Pine Valley Mountains.

The principal tributaries of the Colorado from the region under discussion, commencing on the west, are the Rio Virgen, the Kanab, Tapete River and the Paria. All of these rivers, for the greater part of their courses, run in deep gorges, and this is true also of their tributaries; so that the region is traversed by a labyrinth of profound cañons.

From a line some distance south of the Grand Cañon, to a line somewhat north of this region, all the geological formations have a general dip to the north. To the south, the upper formations have been eroded away, and in going from the bottom of the Grand Cañon north to the plateaus in which the streams mentioned have their sources, we pass over the upturned edges of nearly 25,000 feet of geological formations. Commencing below, in the most southern bends of the Grand Cañon, we find about 1,000 feet of metamorphic crystalline schists, with dykes and beds of granite. In the lateral cañons, which enter from the north, we discover another group of rocks, composed chiefly of sandstones and shales, with varying local dips. Dykes of eruptive rocks penetrate these beds in many places, or pass through them in others, and large accumulations of igneous material are found. This group is non-conformable with the lower metamorphic rocks, for wherever the junction has been seen there are abundant evidences of extensive erosion intervening. The rocks are of Pre-carboniferous age. No fossils have been found in them, but the Carboniferous rocks lie on their upturned edges, so that there was a long period of erosion separating these formations also. The Carboniferous sandstones, limestones, and shales, next succeeding, are from 4,000 to 5,000 feet in thickness; then we have about 2,500 feet of what are deemed to be Triassic rocks; next we have 1,000 or 1,200 feet of Jurassic rocks; still surmounting these, we have 1,800 or 2,000 feet of Cretaceous beds, and then we reach Tertiary rocks, 3,000 or 4,000 feet in thickness in this district, but farther to the north obtaining a thickness of nearly 7,000 feet.

The most remarkable features of the country are the deep narrow cañons by which it is interrupted, making its exploration a task of no little magnitude. The cañon walls are towering escarpments of rock, that in many places cannot be scaled.

The class of features next in importance consists of long lines of cliffs which stand athwart the country. These cliffs are bold escarpments of rock, often many hundreds of feet in height, and scores or hundreds of miles in length; in many places quite vertical above, with a sloping *talus* at the foot. There are two series of these cliffs in the region under discussion, having their origin in very different causes. The cliffs belonging to one of the series have an easterly and westerly trend; the other a northerly and southerly.

The lines of cliffs which extend across the country in an easterly and westerly direction will be discussed first. Starting at the brink of the Grand Cañon and going to the north, we travel on the summit of Carboniferous formations, until we reach a line of cliffs from 100 to 400 feet in altitude. This escarpment is capped by a firmly cemented conglomerate containing many fragments of silicified wood, and over its surface are scattered many like fragments, and sometimes huge tree-trunks, which are the remnants of rocks at one time overlying the conglomerate, but now carried away by erosion. Underlying this cap are variegated sandstones and marls. The whole group is probably of lower Triassic age. The silicified woods so abundant here are called by the Indians who inhabit the country, Shin-ar-ump; or, The arrows of Shin-aú-av. (Shinauav is the Hercules of their mythology.) To the cliffs they give the name Shin-ar-ump Mu-Kwan-i-Kunt, and we have adopted as the English name, Shin-ar-ump (or Arrow) cliffs. Still passing to the north a few miles, we reach the foot of a second line of cliffs, composed of red sandstone, and beds of lighter color, which are stained red on the surface. To this line the Indians have given the name Un-Kar Mu-Kwan-i-Kunt; we have adopted the translation, *Vermilion Cliffs.* This escarpment is often more than 1,000 feet in altitude, bold in outline and brilliant in color, and forms a conspicuous feature on the landscape. Still going to the north for a half day's journey or more, we reach the foot of another line of cliffs, which are capped by nearly two hundred feet of limestone, beneath which is found a bed of massive light-grey sandstone. The Indian name for this line is, At-sí-gar Mu-Kwan-i-Kunt, or, Gray Cliffs, which English equivalent we have adopted. The limestone at the summit contains Jurassic fossils.

Continuing to the north, Cretaceous beds are seen in the hills on either side; farther to the east these beds are found in two lines of cliffs, of which no farther mention will here be made. Having passed the Cretaceous rocks, and many hundreds of feet of lower Tertiary, we reach the foot of another line of cliffs composed of limestones and sandstones; some of these beds are of light pink color, and the exposed edges of all are stained in such a manner as to give the same bright color to the entire face of the escarpment. To this line we have given the name Pink Cliffs, being the English translation of the Indian name, Un-tsaw-ar Mu-Kwan-i-Kunt.

The general dip of all these formations, it will be remembered, is to the north, and in passing from one line of cliffs to another, where the same stratum is followed, a gradual descent is made; but the traveler in going north is almost imperceptibly passing on to later formed beds, until he meets with a line of cliffs where the ascent is made abruptly. The Shinarump Cliffs have been traced from a point west of the Rio Virgen to a point many miles east of the Colorado River,—a distance of nearly 200 miles; the Vermilion Cliffs have been traced somewhat farther, as have the Gray Cliffs. The Pink Cliffs are about 50 miles in length, and form the southern boundary of two extensive plateaus that are separated by Long Valley and the Valley of the Sevier, and have a general altitude above the sea of eight to nine thousand feet.

It will be seen from this description that to go from the bottom of the Grand Cañon to the summit of these plateaus, you must climb by a great geographical stairway, the steps of which have an attitude of many hundreds of feet, and a width of many miles. As the rocks dip to the north, the difference in altitude between the two points is only about 7,000 feet; but were the beds horizontal, the plateau would be more than twice that height above the river.

It is a remarkable fact that the geological formations do not terminate by a thinning out of the strata, or by gently eroded slopes, but most of them end abruptly in lines of cliffs; and where these are found it is invariably seen that the summits of the escarpments are composed of rocks of a homogeneous and firm texture, with underlying rocks that yield more readily to atmospheric degradation, that is, the rocks below are of heterogeneous composition, and are not friable.

Another of the important conditions under which these wonderful cliffs have been formed, is this: that the progress of the emergence of the folds above the general surface of the country, or level of the sea, was, in its earlier stages at least, little or no greater than the progress of erosion, so that the rocks were carried away quite as fast as they were exposed to denudation. It would be beyond the reasonable limits of such an article as the present to discuss this subject.

The lines of cliffs which have a northerly and southerly trend are due to abrupt displacements of the strata, either by faulting or folding. I propose to call these displacements broken folds, for reasons which will subsequently appear.

On the east side of the Grand Wash we discover a great fault extending across the Colorado River. The drop of the fault is on the west side of the fracture. The wall remaining *in situ*—for I supposed it to be a drop, not an uplift—stands in a bold escarpment which forms the eastern wall of the Grand Wash. To the escarpment we have given the name, Grand Wash Cliffs, and to the displacement, Grand Wash Fault.

Farther to the east another fault is discovered, approximately parallel to the first; the drop of the bed is also to the west. It extends from an unknown point south of the Colorado, in which direction it has been traced about thirty miles without discovering its terminus, to a point north of Tokerville. Its northern terminus has not yet been discovered. The displacement is from 1,300 to 2,800 feet. To the south it is a fault, but farther to the north it is seen to change gradually to a monoclinal fold. The broken edges of the rocks on the eastern side of the fault, which have not been displaced, stand in a remarkably steep escarpment, in much of its course a sheer precipice, impossible to be scaled even by men accustomed to mountain climbing. Several small towns have been located along its foot, and the people have given to the cliffs lying to the south of the Rio Virgen, the name Hurricane Lodge, but in order to conform this to my general nomenclature, I have called it Hurricane Cliff. The line of cliffs north of the Rio Virgen we designate as Toker Cliffs; the displacement we call Hurricane Fault.

It will be observed that the direction of these faults is, in a general way, at right angles to the grand strike of the formations, and as the drop is to the west of the fracture, the local dip is easterly.

Going yet farther to the east about twenty miles, another fracture is discovered. This has been seen to extend south of the cañon twenty-five or thirty miles; how much farther it may continue is not known. It has been traced to the north through the Vermilion Cliffs, where Short Creek Cañon marks its position. Where it crosses the Shinarump Cliffs, the displacement is seen to be about 120 feet. On the north side of the Grand Cañon it is marked by a cañon valley about thirty miles long, to which we have given the Indian name, To-ró-weap. At the foot of the valley, on the brink of the Grand Cañon, the displacement was found to be 820 feet, and it appears to be still greater on the south side. We have named this To-ró-weap Fault, and to the clifts have given the same name. Again to the east another fault is discovered. We are not yet certain whether this extends to the south of the Grand Cañon or not; the most southern point where it has been seen is about ten miles north of the cañon, from which point it has been traced past Pipe Spring to the foot of Long Valley, thence up Long Valley to its head, and from thence across the divide to the head of Sevier Valley, down the Sevier Valley to Salina, and still continuing in a northerly direction up the Sanpete Valley to its head opposite Mount Nebo. The drop of the rocks is still on the western side of the fissure, and varies from 100 to 3,000 feet. In some places this displacement also appears as a monoclinal uplift. We have named this the Fault of the Sevier. Along its course several lines of cliffs are seen which have been designated as follows: the line on the east side of Sanpete Valley, Sanpete Cliffs. From the foot of Sanpete Valley to a point about midway up the valley of the Sevier, ranges of eruptive mountains are seen to the east, the rocks constituting which have probably emerged from the fissure of this fault, and have, to some extent, obscured it. South of these eruptive ranges, the eastern wall of the valley of the Sevier is a well marked line of cliffs, which forms the western boundary of one of the plateaus heretofore mentioned. To this line we have given the name Sevier Cliffs. Another line is seen on the eastern side of Long Valley, named Long Valley Cliffs. Another fault is seen at the cañon of the Kanab. It has been traced about thirty miles; the drop is from 100 to 200 feet, and still on the west, but being inconsiderable, no well marked line of cliffs has been formed; this we call Karab Fault.

Another fault to the east marks the western boundary of a great plateau; this displacement, either as a fault or monoclinal uplift, has been traced to the northern sources of the Dirty Devil River, and it probably extends much farther; to this we have given the name Western Kaibab Fault. The eastern edge of the same plateau marks the line of another fault. This has been traced about fifty miles south of the Grand Cañon, and probably extends still farther, as a continuation of the line would strike the San Francisco Mountains, a vast group of volcanic tables and cones, and it is conjectured that the eruptive matter issued from the fissures of this fault and its branches; to this we have given the name Eastern Kaibab Fault; and like the Western, it has been traced far to the north, and it is believed to extend to Price River Valley; but here the drop changes and is found on the eastern side of the line.

Still other folds and faults have been found to the east, but none of them have been traced, having been seen only at points along their lines, and hence no farther mention of them will here be made.

This great system of broken faults has been a subject of much careful study. Each one has been traced along its line through the whole extent of the country under discussion. From time to time the drop had been measured, and a great variety of accompanying phenomena observed. Some of these facts are of much interest. In many places the faults are seen to branch; in others they suddenly or gradually change into monoclinal uplifts; in still others the drop marks but a part of the displacement; the edge of the fallen rock having caught on the wall remaining *in situ*, is turned up, so that below it appears as a fold, and above as a fault. In other places the edge of the fallen rock is bent down, and in still other places the rocks are not separated by well defined fissures, nor are they folded, but irregular masses of broken rocks intervene.

It is interesting to observe the way in which these two systems of cliffs are related. The eastern and western faults are of very irregular outline; sharp salients are set out on to the plains below, and deep re-entering angles are seen, and streams that head to the north and flow to the south, have cut cañons through the cliffs, so that it is possible to ascend these great steps by passing up a cañon way, rather than by climbing escarpments. Wherever a fault crosses one of these lines, the latter is broken, and where the drop of the fault is to the west, the line of cliffs on the western side of the fault is thrown to the south, to a distance which is in direct ratio to the extent of the drop.

The bearing of these facts, in the study of the conditions under which these cliffs were formed, is very interesting, but I may not stop to discuss it farther here.

The lines of cliffs which are formed by the north and south faults are of much more regular outline, and are more rarely crossed by cañons, yet, in a few places they are thus cut by channels of streams. In some places these streams, in crossing the fault, run from the upper to the lower beds. In other places they run from the lower to the upper beds.

The next group of topographical features in this country, consists of the plateaus, to some of which I have heretofore alluded. East of the Grand Wash Cliffs, and west of the long cañon valley at the foot of the Hurricane Cliffs, and north of Grand Cañon, and south of a short abrupt fold that can be seen to extend between the two faults a little south of Fort Pierce, there is a great table, its surface having an inclination to the northwest, determined by the grand dip of the rocks to the north, and the local dip to the east, which is due to the faulting. To this we have given the name Sheáv-wits Plateau.

Between the Eastern and Western Kaibab Faults there is an extensive plateau, extending from the Grand Cañon to the foot of the Vermilion Cliffs; the Indian name for this is Kaibàb, meaning, Mountain lying down, and pleased with its significance, we have adopted the name Kaibab Plateau.

A triangular table of Triassic sandstone is seen between the Colorado, the Paria, and House Rock Valley; for this we have adapted the Indian name, Un-kár Kaiv-áv-i. The Indian name for the plateau east of the head-waters of the Sevier is Pouns-á-gunt, meaning, the Home of the Beaver; this name we have also adopted. The plateau west of the river they called *Mar-ká-gunt*, Home of the flowering bushes; I hardly need add that we were pleased to adopt this name also.

Through the great fissures of these faults floods of lava have issued and spread over the country in broad sheets, or run down into and filled the valleys. The earlier sheets of this basalt, being of firmer texture than the sedimentary rocks, have preserved the districts of country over which they have spread from erosion, and thus table-mountains have been formed. In our earlier studies we supposed that some of these mountains were masses of igneous rocks. On farther examination it was found that they are composed of sedimentary beds with caps of basalt only. Since the formation of such beds of basalt, atmospheric agencies have carried away many hundreds of feet of the adjacent softer beds, leaving those portions of the bed, protected by the harder material, as table-shaped mountains. As erosion advanced on the sides of these mountains, the more friable material below was carried away, and the edges of the sheet of basalt undermined, and huge blocks have tumbled down, and, to a greater or lesser extent, obscured the true structure of the mountain. In many places later overflows have occurred in these mountains, not issuing from the summits of such tables, on the flanks, and it may be accepted as a general rule, that the later the flow, the lower the point from which it emerged.

In other regions these sheets of basalt seem to have been formed at a period later than those seen in the table mountains, so that only low mesas appear; but of course no definite line of demarkation can be drawn between such mesas and what we have designated as table mountains.

The expiring energies of these eruptive agencies have left great numbers of cinder cones standing in lines along the fissures. Many of these have well defined craters, and they everywhere form conspicuous features on the landscape.

Cañons and Valleys.

No sharp line of division can be drawn between cañons and valleys. For convenience, we designate intervening depressions caused by erosion, cañon valleys, but all of these excavated basins and troughs will be included under the general head of valleys.

This is a region almost everywhere of naked rock. The cañon walls, and cliffs, present vertical sections of strata of great magnitude, and the nakedness of the upper surface of the rocks, together with the exposure in the escarpments, make it possible to examine the geological structure of the country with great thoroughness; and conclusions may be reached with a degree of certainty elsewhere rarely attainable. Under these circumstances, it has been possible to understand the causes which have combined to determine the vast system of drainage, and to discover the relation in the direction of the valleys to the dip of the folds. I propose to classify the valleys of this country in the following manner:

Order first: transverse valleys, having a direction at right angles to the strike.

Order second: longitudinal valleys, having a direction the same as the strike.

Of the first order three varieties are noticed:

a, monoclinal, those which pass through a fold;
b, acclinal valleys, that run in the direction of the dip;
c, contraclinal valleys, that run against the dip of the beds.

Of the second order we also have three varieties:

A, anticlinal valleys, which follow anticlinal axes;

B, synclinal valleys, which follow synclinal axes;

C, monoclinal valleys, which run in the direction of the strike between the axes of the fold, one side of the valley formed of the summits of beds, the other side composed of the cut edges of the formation.

Many of the valleys are thus simple in their relation to the folds; but as we have two systems of displacements, a valley may belong to one class in relation to one fold, and to another in its relation to a second; such we designate as complex valleys. Again, a valley may belong to one class in one part of its course, and to another elsewhere in its course; such we designate compound valleys. It will farther be noticed that valleys may have many branches, but in relegating a valley to its class we consider only the stem of the valley proper, and not its branches.

A great diversity in the features of all these valleys is observed. Most of these modifications are due to three principal causes: first, a greater or lesser inclination of the rocks; second, the texture of the beds, that is, their greater or lesser degree of heterogeneity; the third class of modifying influences is found in the eruptive beds.

In the country under discussion the opportunities for studying the effects and extent of erosion are very great. The amount of erosion exhibited in the Grand Cañon alone is so great as almost to stagger belief. If a hundred mountains, each as large as Mount Washington, were tumbled into this cañon, they would scarcely fill it; but even this great amount does not furnish a practicable unit with which to measure the denudation of the country. A vast labyrinth of deep gorges has been excavated, extending to every part of this region, and should we compute the amount of rock necessary to fill these to the general level of the country, we should still have but a meagre term of comparison for the sum of the material which has been carried away by rains and rivers.

On the flanks of the folds which are found everywhere in the valley of the Colorado, we see the edges of formations which once, doubtless, extended quite over the folds. That these formations were once continuous appears evident from the following considerations: first, they are not seen either to thin out, or thicken up, and bear no evidences of having been immediate shore formations; second, they terminate in abrupt escarpments; third, they may be traced on either side of the folds, and seem to have the same lithological and paleontological characteristics; and fourth, outliers of the formations may be discovered in many places, that have withstood the vicissitudes of erosion. The most remarkable of these are such as have been protected by sheets of eruptive rocks. In the Minkaret Mountains we find a group of basaltic tables and cones standing far out on the Carboniferous rocks. Twenty-five or thirty miles to the north we are able to study a series of shales, marls, and sandstones, of Triassic age, exposed in the Shinarump Cliffs. The surface of the country intervening between the mountains and the cliffs is seen to be Upper Carboniferous; but here, in the mountains, from 1,200 to 1,500 feet of Triassic rocks can be studied, and it is found that all the details of bedding observed in the cliffs are repeated on the sides of these tables; they are thus proved to be outliers of beds that formerly extended over the intervening region.

The contemplation of this vast extent of erosion will not stagger us, when we reflect that the sedimentary beds are evidences of an amount of erosion co-extensive with the magnitude of these formations, and anterior to that which we are now studying; but it is interesting to study erosion by observing its immediate effects, rather than examining beds which only give evidences that such an amount of erosion actually occurred,—without being able to trace it farther.

The general history of the geological operations recorded in this desolate waste can be briefly summed in the following statement: The deposition of beds, of many thousands of feet in thickness, in which are enclosed the relics of life which existed when these rocks were formed, and which were buried in their accumulating sands; then the folding and faulting of the whole series, and *pari passu* with this, the excavation of a wonderful system of gorges, and the carving of a vast net-work of valleys, leaving behind towering cliffs stretching across the country in every direction, and still, *pari passu*, with the folding, and faulting, and denudation, great floods of lava were poured out from the interior to fill valleys, and form mesas, and tables, and mountain cones. And in the present period we have an *ensemble* of topographical features embossed on the face of the country, wild, grand, and desolate.

ART. LI.—*Remarks on certain Errors in Mr. Jeffreys's Article on "The Mollusca of Europe compared with those of Eastern North America;"* by A. E. VERRILL.*

IN the October number of the Annals and Magazine of Natural History, Mr. Jeffreys published an article upon this interesting subject, in which many important errors occur, due, no doubt, to the fact that the distinguished author is much less familiar with American than with European shells. But as the dredgings in connexion with the investigations of our fisheries by the U. S. Fish Commission were under my superintendence during the two past seasons, and Mr. Jeffreys alludes to the fact (though rather indefinitely) that he, by invitation of Professor Baird, accompanied us on several dredging-excursions in 1871, it seems necessary that I should point out some of the more important of these errors, lest it be supposed by some that the same views are held by me.

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* From the Annals and Magazine of Nat. Hist., IV, vol. xi, p. 206.

It is not my intention to discuss at this time the numerical results presented by Mr. Jeffreys; but I would remind the readers of his article that the regions compared are in no respect similar or parallel, and that it is scarcely fair to compare the shells from the entire coast of Europe with those from about 200 miles of the coast of New England, where the marine climate is for the most part more arctic than that of the extreme north of Scotland—and, moreover, that the last edition of Gould's "Invertebrata of Massachusetts" contains only a part of the species added to our fauna since the first edition was published in 1841, and very little of the great mass of facts in regard to distribution, &c., which have been accumulated by American naturalists during the last thirty years. Consequently that work is far from being a good "standard of comparison." To make a just comparison, all the shells on our coast, from Labrador to Florida, should be compared with those of Europe.

And without going into a long discussion of his peculiar views on the geographical distribution of our shells, I would remark that, to an American, it seems rather singular that most European writers, whether zoölogists or botanists, find it necessary to trace back to a European origin all the existing species of this country, and to suppose that they have "migrated" from Europe to America and other countries in spite of opposing currents and all other obstacles. Thus Mr. Jeffreys can imagine that our land and freshwater shells could have migrated from Europe all the way across Asia, the Pacific Ocean and North America in order to reach Canada and New England; but he does not seem to think it possible that they may have *originated* in America, and thence crossed to Europe in the direction of the prevailing currents and winds. Nevertheless geology teaches us that America was a great continent, in very early ages, when Europe was only a group of islands; that no other country is richer in the remains of terrestrial animals and plants connecting the Tertiary and Cretaceous ages with the present; that many of these supposed European forms (whether terrestrial or marine) can be traced back into our Tertiary formations quite as far (if not farther) than they can in Europe; and that many of the genera of animals, and especially of plants, now found living in both countries, can be traced back to the Cretaceous in America and only to the Tertiary in Europe. Moreover the great number and diversity of the land and fresh-water shells of America (*e. g.*, of *Unionidæ*, *Melaniæ*, &c.), and the peculiar facts in their geographical distribution, cannot but convince any one familiar with the subject that they have *originated* in America at a very remote period; which is confirmed by the fact that many of these can be traced far back into our Tertiary formations. Nor are there sufficent reasons for supposing that those of our species living also in Europe have had a history different from those that are still peculiar to America.

Of course, no one will deny that certain species of land-shells have been introduced from Europe in modern times by human agency; but, so far as most of the identical species are concerned, it seems to us far more probable that America gave them to Europe, rather than the contrary, and this whether animals or plants, terrestrial or marine.

But the special errors to which I wish to call attention occur in the table of species, showing their geographical distribution. These relate both to the names and specific identity of certain shells, and to their geographical distribution. Although not agreeing with the author in regard to many of his remarks concerning the generic relations and names of species, I do not propose to discuss them here; for there seems to be no danger of their general adoption, either in Europe or America.

The following marine species (named as in Gould), which Mr. Jeffreys puts down as belonging to the region north of Cape Cod, actually belong properly to the region south of Cape Cod, extending in most cases to the Carolina coasts or beyond, while north of Cape Cod they are rare or local, viz :—*Cochlodesma Leanum*, *Mactra lateralis*, *Petricola pholadiformis*, *P. dactylus*, *Gouldia mactracea*, *Cytherea convexa*, *Venus mercenaria*, *V. notata*, *Gemma gemma*, *Liocardium Mortoni*, *Arca transversa*, *Modiola plicatula*, *Pecten irradians*, *Ostrea Virginiana*, *Anomia electrica* (not of Linn.), *Diaphana debilis*, *Cylichna oryza*, *Placobranchus catulus*, *Crepidula fornicata*, *C. plana*, *C. convexa*, *C. glauca*, *Ianthina fragilis*, *Bittium Greenii*, *Odostomia bisuturalis*, *O. seminuda*, *Turbonilla interrupta*, *Pleurotoma bicarinata*, *P. plicata*, *Nassa obsoleta*, *Buccinum cinereum*, *Diacria trispinosa*, *Loligo Pealii.*

The following, to which a northern distribution is likewise given, are also found far south of Cape Cod, and many of them belong quite as much to the southern as to the northern division; and some of them are decidedly southern, extending even to the Gulf of Mexico :—*Teredo navalis*, *T. megotara*, *T. chlorotica*, *Solen ensis*, *Machæra costata*, *Pandora trilineata*, *Lyonsia hyalina*, *Mac-*

tra solidissima, *Kellia planulata*, *Macoma fusca*, *Tellina tenera*, *Astarte castanea*, *A. quadrans*, *A. sulcata*, *Nucula proxima*, *Yoldia limatula*, *Mytilus edulis*, *Elysia chlorotica*, *Crucibulum striatum*, *Littorina rudis*, *L. tenebrosa*, *L. palliata*, *Lunatia heros*, *L. triseriata*, *Nassa trivittata*, *Melampus bidentatus*, *Alexia myosotis*.

Many others, not named in the above lists, are not limited by Cape Cod; but as they belong properly to the northern division, they are here omitted.

As an offset of these numerous instances in which he has unduly exaggerated our northern fauna, we find not one undoubted instance of an error on the other side, among the marine shells.

The distribution indicated for our land and freshwater shells is even more erroneous. It is sufficiently evident that Cape Cod is in no sense a proper boundary between the northern and southern fluviatile and terrestrial species; but, disregarding this, there are no reasons whatever for most of the special indications that he gives.

Thus he gives the northern distribution to all of the sixteen species of *Sphærium* and *Pisidium*; but most of them are well known to be widely distributed over the eastern, middle, and western parts of the United States, some even extending to the southern parts. *Unio complanatus*, *U. nasutus*, *Margaritana arcuata*, and *Anodon implicatus* are indicated as distributed north of Cape Cod; but all these are found over most of the northern and middle states and some in the western, while the last one is somewhat rare at the north. But *Unio radiatus*, *U. cariosus*, *U. ochraceus*, *Margaritana undulata*, *M. marginata*, *Anodon fluviatilis*, and *A. undulatus* are put down as southern. It would certainly be difficult to show that these, as a group, are more southern than the previous lot; for most of them have nearly the same wide distribution, and all of them, except *U. cariosus*, occur even in Maine. Some of them (as *U. radiatus*, *M. undulata*, and *A. fluviatilis*) are the most abundant species in all the waters of northern New England and New Brunswick. The distribution given for the species of *Valvata*, *Melantho*, and *Amnicola* is equally faulty.

All of the eighty-one species of *Helix*, *Hyalina*, *Macrocyclis*, *Limax*, *Pupa*, *Vertigo*, *Succinea*, *Arion*, *Zonites*, *Tebennophorus*, *Limnæa*, *Physa*, *Bulinus*, *Planorbis*, and *Ancylus* are set down as having the northern distribution, except *Hyalina Binneyana*, *Pupa fallax*, *Limnæa catascopium*, and *Physa ancillaria*. But every American conchologist knows that nearly all of those species are very widely distributed over North America, east, west, north, and south, many of them being limited only by the Gulf of Mexico on the south and California or the Pacific on the west. Nor is there any reason for the distinction made in the case of the four species named above; for these, though differing among themselves, have the same distribution as many of those put down as northern, while *H. Binneyana* and *P. ancillaria* certainly have a very northern range, for they are abundant in Maine, New Brunswick, and Canada.

It is evident that such numerous errors of this kind render the paper, so far as geographical distribution is concerned, quite worthless; for it is sure to mislead.

Most of these errors might have been easily avoided had the author depended less on Gould's work and more on the recent works of American conchologists; for there is no lack of data in regard to the distribution of most of our shells. Even Dr. Stimpson's "Shells of New England" (1851), if consulted, might have saved most of the errors in regard to the distribution of the marine shells.

The fact that there is in the southern and shallower parts of the Gulf of St. Lawrence an isolated colony of southern shells, may have misled Mr. Jeffreys in many cases, especially as he evidently consulted the Canadian collections much more than those of the United States, many of the largest of which he did not see at all. In respect of erroneous identifications and the reduction of certain species to varieties, there is also much to be said; but this article is already so long that it will be necessary to refer only to some of the more obvious and important errors of this kind, leaving the rest to be discussed more fully elsewhere.

Every naturalist should be willing to allow his fellow naturalists full liberty of opinion with respect to the specific identity or difference of closely allied forms; and no one can claim to be infallible in such matters. Some of the errors to be mentioned do not, however, come under this head; for the species united have only remote affinities. Nevertheless the naturalist who has collected and carefully studied animals in their native haunts, under various circumstances, in many localities, and in great numbers, has, other things being equal, a very great advantage in these matters; and therefore I believe that Mr. Jeffreys would in most cases agree with me had he collected and studied as many American shells as I have, during the past fifteen years, or if he were as familiar with them as he is with the British species. In most of the cases to which I refer, my own conclusions are in harmony with those of Dr. Stimpson, who devoted so many years to collecting and carefully studying our shells, and who is well known for his accuracy in such matters. And it would be strange indeed if all American naturalists, as well as many eminent foreign ones, have always been making such ridiculous blunders in regard to some of our most familiar shells as Mr. Jeffreys would have us believe.

Thus he states (p. 240) that "*Gemma gemma*" (or *Tottenia gemma*) is the young of *Venus mercenaria!* But it has long been known to European as well as American conchologists that the animal of *gemma* is very different from that of *mercenaria*, and quite peculiar; that the hinge is constructed on a very different type is well known; and Prof G. H. Perkins has shown (Proc. Bost. Soc. N. H., 1869, p. 148) that *gemma* is *viviparous*, producing about three dozen young, with well-formed shells, at one time. Moreover, the young shells of *mercenaria*, smaller than the adult *gemma*, are sufficiently abundant on our shores, and may be seen in many American collections; they are certainly very unlike the *gemma* in form, sculpture, and hinge, as has been well known for more than thirty years.

Again, he states that *Arca transversa* is a variety of *Arca pexata*, the former being put down as northern, the latter as southern. That these shells are widely different in form and in the structure of the hinge is well known; for Dr. J. E. Gray many years ago established a new genus (*Argina*) for the latter, on account of its very peculiar hinge. That the animals are also quite different I can assert from personal observation. Moreover, the differences in the hinge, epidermis, and form are remarkably constant; and, finally, the two species have the same geographical range from Cape Cod to South Carolina, and are often found together. Both are very common in Long Island Sound and New Haven harbor; and I have examined hundreds of specimens of both species without finding the slightest evidence in favor of Mr. Jeffreys's views. Indeed, they are only distantly related, and evidently belong to distinct genera, *Argina* and *Scapharca*, where several writers have placed them.

He also states that *Mactra ovalis* is a variety of *M. solidissima*. He may not have seen a specimen of the true *ovalis*, for it is not common in collections; but the genuine *ovalis* is certainly a very well-marked species, widely different from the *solidissima*. They differ greatly in the hinge, epidermis, form of shell, and position of the umbos; moreover, the animals are also quite different. Both occur together of equal size in the Bay of Fundy; but the former is not known south of Cape Cod, while the *solidissima* is abundant everywhere along our sandy shores to South Carolina.

Concerning *Astarte castanea* he says, "Perhaps a variety of *A. borealis* Ch.;" but *castanea* is one of the best-defined species in this difficult genus, varies comparatively little, and does not extend far north, its range being decidedly *southern*. It is perfectly distinct from *A. borealis*. He reduces *A. quadrans* to a variety of *A. castanea*, and gives it a name that is quite uncalled for, even if this view were correct. He then makes *A. Port-*

landica a variety of *A. compressa;* but I have already shown (Amer. Journ. of Science, April, 1872) that it is a variety of *A. quadrans.* His arrangement of the other species of *Astarte* is equally objectionable, but it is not necessary to discuss them here.

The *Pecten fuscus* Linsley is given as the young of *P. irradians*, from which it is very distinct; but the writer has shown (Amer. Journ. of Science, vol. ii, p. 361, and vol. iii, p. 213, 1871–72) that it is really the young of *P. tenuicostatus.*

Dekay is given as the authority for *Æolis salmonacea* and *Æ. gymnota;* but they were both described by Couthouy in 1838, from whom Dekay borrowed both the descriptions and figures, five years later.

He states that *Dentalium dentale* (non Linn.) is a variety of *Entalis striolata*, and that the latter is a variety of *D. abyssorum* Sars; but both of these statements are incorrect. The first is the *Dentalium occidentale* Stimpson, and is a true *Dentalium*, entirely different, generically and specifically, from the *striolata;* and the latter is also quite distinct from *abyssorum.* Possibly Mr. Jeffreys has not seen perfect specimens of all the American species; otherwise, I cannot understand how he could have made these statements.

He is correct in considering *Crepidula glauca* a variety of *C. fornicata*, as others have done before him; but he has adopted a serious mistake, made by several other writers, in regarding *C. plana* (or *unguiformis*) also as a variety of *C. fornicata*, from which it is really very distinct. It is a very common error to suppose that this species always inhabits the inside of dead univalve shells; for it very often occurs on the *outside* of such shells, on stones, the back of *Limulus*, &c., and is frequently associated intimately with *fornicata* in all these situations; but nevertheless it always retains its essential characters, under all circumstances. The typical *fornicata* is also often found with it, plentifully, on the *inside* of dead shells.

Nor can *Margarita acuminata* be the young of *M. varicosa;* for in our collection there are full-grown specimens of both, equal in size, from Labrador.

There is no sufficient reason for adopting the name *Lacuna divaricata* in place of *L. vincta*; for it is not the *Trochus divaricatus* of Linné (1767), although it is the shell described under the same name by Fabricius in 1780, as shown long ago by Dr. Stimpson and others. Fabricius made a mistake which we have no right to perpetuate; nor does "usage," to which Mr. Jeffreys so often appeals, sanction the change.

The *Lunatia triseriata* is not, as Mr. Jeffreys thinks, the young of *L. heros*, but only a color-variety, as the writer had previously shown (April, 1872). Both varieties occur together, from the smallest to the largest sizes; but the former sometimes becomes plain-colored before reaching maturity. There is no evidence that *Natica clausa* is the *Nerita affinis* of Gmelin, but quite the contrary; for the latter was placed in the section of *umbilicated* species, was described as *silvery within*, and came from New Zealand! It is probably one of the Trochidæ, and certainly could not have been this *imperforate Natica.*

In this place I shall not enter into a discussion of the numerous cases in which the author has reduced the American shells to "varieties" of the European species, because in many of these cases there must long be great diversity of opinion, and for most purposes it matters little whether these closely related forms be called "varieties" or "species," so long as the actual differences are recognized. But since Mr. Jeffreys has evidently made so many important mistakes in his article in regard to the identity of species, and has united those that have no near affinities, as already shown, it is logical to conclude that he may have made other mistakes in the case of more critical species. He must therefore pardon us if we regard his decisions in all these cases as at least doubtful, until confirmed by other evidence.

Art. LII.—*Note on the use of a diffraction "grating" as a substitute for the train of prisms in a Solar Spectroscope;* by Prof. C. A. Young.

Since the diffraction spectrum differs from a prismatic spectrum of the same length in having the less refrangible rays more widely dispersed, it some time ago suggested itself that a so-called "*gitter-platte,*" or "grating" of fine lines, might advantageously replace the prisms in spectroscopes designed for the observation of the solar prominences through the C line. In this idea I was strongly confirmed on seeing last winter some of the beautiful gratings ruled upon speculum metal by Mr. Chapman, Mr. Rutherfurd's mechanician. The spectra furnished by these plates far exceed in brilliance and definition anything of the kind ever before obtained.

Through the kindness of Mr. Rutherfurd I have recently come into possession of one of them, having a ruled surface of something more than a square inch, the lines being spaced at intervals of $\frac{1}{6480}$ of an inch. Combining this with the collimator and telescope of a common chemical spectroscope we get an instrument furnishing a spectrum of the first order in which the D lines are about twice as widely separated as by the flint glass prism of 60° belonging with the original instrument. In the neighborhood of C the dispersion is nearly the same as would be given by four prisms.

The spectra of the higher orders are generally not so well seen on account of their overlapping each other, but fortunately with one particular adjustment of the angle between the collimator and telescope, the C line in the spectrum of the third order can be made to fall in the vacant space between the spectra of the second and fourth orders, and we thus obtain an available dispersion nearly the same as that of the instrument I am accustomed to use.

On applying the new instrument to the equatorial, I found (under atmospheric conditions by no means favorable, though the best that have presented themselves as yet), that in the first order spectrum I could easily see the bright chromosphere lines C, D_3, and F; I could also, though with great difficulty, make out $H\gamma$, (2796 K). On opening the slit the outline of the chromosphere and the forms of the prominences were well seen, both in the spectra of the first and third order, quite as well I think as with my ordinary instrument in the same state of the air. The spectra are of course fainter, but as this loss of light affects the back ground upon which the prominences are projected, as well as the objects themselves, it does not materially injure their appearence.

The grating is much lighter and easier to manage than a train of prisms, and if similar ruled plates can be furnished by the opticians at reasonable prices and of satisfactory quality, it would seem that for observations upon the chromosphere and prominences they might well supersede prisms.

Dartmouth College, May 9, 1873.

SCIENTIFIC INTELLIGENCE.

I. GEOLOGY.

1. *Note on the occurrence of the Trias in British Columbia;* J. D. WHITNEY. From a letter to one of the editors, dated Cambridge, Mass., May 7, 1873.—Several years ago, one of the officers of H. M. Surveying ship Hecate brought to the office of the California Survey a slab of rock, which had been collected somewhere up the coast far to the north of San Francisco, but the exact locality of which I was unable to ascertain, as the specimen was left in my absence. The slab was to me especially interesting as it closely resembled, both lithologically and paleontologically, our Plumas County Triassic slates.* Indeed the specimen looked so familiar that for some time I could hardly convince myself that there was not some deception about it.

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* See Geol. of Cal., Vol. i, p. 309.

It was on the strength of this specimen that I ventured to extend the range of the Alpine Trias as far as British Columbia, in the little sketch I sent of our results, and which was published in this Journal for August 1864 (vol. xxxviii, p. 261). I did not fail to impress on Mr. Dall, when he started for Alaska, the importance of keeping a sharp look-out for the fossils of this interesting formation. He was not, however, so lucky as to fall in with any fossiliferous deposits of importance, nor was he able to throw any light on the occurrence of the specimen brought by the Hecate.

M. Pinart, a French explorer, however, has been more fortunate, since it appears from a communication made by M. Fischer to the French Academy of Sciences, last December (See Comptes Rendus, lxxv, 1784), that M. Pinart collected specimens from the entrance of Pavalouk Bay, which were densely crowded with a species of *Monotis*, and which M. Fischer refers to the Alpine Trias, thus in all probability, extending the range of this interesting formation, not merely as far as British Columbia, but even to the Alaskan peninsula. It is possible that the Hecate's specimen was from the very region visited by M. Pinart. This is, indeed, the more probable since, in spite of all my inquiries, I have never yet been able to learn of any fossiliferous rocks cropping out along the coast of British Columbia, or any where on the mainland north of our boundary.

M. Pinart also brought from the Alaskan peninsula (locality Aniakchak and another bay near Mount Chiginagak) specimens of rock containing *Aucella*, in all probability of Jurassic age, this formation having been previously recognized in that region by Grewingk. As the *Aucella* is the most abundant and characteristic fossil of the Jurassic slates of the gold-region of California, this occurrence is also not without interest to us. Judging from the discoveries of Messrs. Grewingk and Pinart, there is a good field for paleontological investigation, as well as for the study of volcanic phenomena, on the Alaskan peninsula and among adjacent groups of islands.

It is interesting to notice how this remarkable grouping of fossils which characterizes the Alpine Trias, and which seemed for a time to have such a limited range, has now been traced all around the world, New Zealand, New Caledonia, the Pacific coast of North America, High India, Spitzbergen; these are localities in which the peculiar *Monotis*- and *Halotia*-bearing slates have been found within the past ten or fifteen years.

2. *Notes to page* 438, *on mountain-making;* by J. D. Dana.—(1.) Although no case of unconformability between the Carboniferous and the underlying Paleozoic is yet distinctly made out in the Sierra Nevada, the Great Basin, or the Wahsatch, such occur farther south according to Mr. J. W. Powell, in the vicinity of the Grand Cañon of the Colorado. (See page 457 of this volume.) The fact that Whitney has found no rocks lower than Carboniferous in the Sierra may be a consequence of the same uncon-

formability beneath these mountains. But in the region of the Cañon, the Carboniferous, Triassic, Jurassic, Cretaceous and Tertiary beds are all conformable. The epochs of mountain-making over the Pacific slope south of the latitudes of the Wahsatch Range, and also of that of the north, were different from those within these latitudes.

(2.) Mr. James T. Gardner, in a letter of May 8th, informs me that in his opinion all the more important mountain ranges of the Great Basin (in Nevada) are included in a chain trending about N. 40° E., the whole having a breadth across the trend of 120 miles. Austin, in Nevada, lies near the center line of the chain. To the west of this elevated region is the great depression where the rivers of Nevada evaporate in Carson, Humboldt, Pyramid, Mud, and other lakes; and to the east is the great depressed area of Salt Lake. On this view, if these mountains were made at the close of the Jurassic, there were formed at this epoch three lofty synclinoria, the Sierra, the Humboldt and the Wahsatch, or the western-border, central and eastern-border chains of the Great Basin. The precise determination of the epoch of origin of the Humboldt chain is therefore of much importance.

3. *Geological Survey of the Territories in charge of Dr. Hayden, under the Department of the Interior. Organization and plans for the year* 1873.—The Department of the Interior U. S. Geological Survey of the Territories, in the charge of Prof. F. V. Hayden, was reorganized last winter and placed in condition to carry on systematic and connected surveys with a permanent corps of assistants. Congress changed the name to Department of the Interior U. S. Geological and Geographical Survey of the Territories, as it has been found desirable to have topographical work conducted under the influence of geologists. Variations of the surface forms are so indicative of structural changes in the western mountains, that the geologist uses topography as one of the principal branches of his science. The experience of the 40th Parallel Survey has been that the opportunities for mutual criticism and discussion between the gentlemen representing the two departments of geology and topography have been of the greatest advantage to both.

The action of Congress in uniting these two branches of natural science under one organization promises the best results. The scientific corps of the Survey now consists of Prof. F. V. Hayden, Geologist-in-charge, and his staff of three assistant geologists, several distinguished paleontologists, a botanist, a zoölogist, a photographer and an artist; a geographer and his staff of three topographers; three assistant topographers, a meteorologist, and a draughtsman; a quartermaster and his assistant.

The field of operations authorized by Congress for the coming season is the Territory of Colorado, and that part of Utah lying east of the Green River. It is bounded on the north by the belt of the 40th Parallel Survey, and the primary triangulation will be a part of the same system carried across from the Sierra Nevada by that survey. The work will be based upon a trigonometric survey connected with measured bases. Several of the principal geodetic stations will be determined astronomically by the U. S. Coast Survey, whose experience and training as field astronomers renders their work above all question.

The area to be examined is divided into three districts; the detailed survey of each being entrusted to separate parties, consisting of an assistant geologist, a topographer and assistant, and a naturalist. The geologist is responsible for the report on the geology of his district, and has full credit for it as independent work. The topographer carries the secondary triangulation, and with his assistant works out the topography.

The chief geologist, with an independent party, makes such general and special examinations of the country as are necessary for so great a scientific work. The geographer carries on the primary triangulation and superintends the business and work of the field parties.

The head quarters of the Survey will be at Denver, Col. Ter., from which point the field parties will be supplied by the quartermaster.

First operations will be commenced about the middle of May, and it is hoped that the Survey will soon be able to give to the country accurate maps and descriptions of that most interesting region of the Parks of Colorado, and their encircling groups of snowy summits.

4. *Volcanoes of Hawaii.* Copy of a letter from Rev. TITUS COAN to Prof. LYMAN, dated Hilo, Hawaii, Feb. 14th, 1873.—"You have seen an account of the eruption within the great summit crater of Mauna Loa in August, 1872. This continued for two or three weeks. On the 27th of January of the present year, we had the grandest display from the crater that I have ever seen. The action within it was vehement, and the scene marvelously brilliant. The great mural pit was in awful ebullition, and so violent was the raging of the molten sea within, that herdsmen of Reed and Richardson's ranch, on the eastern slope, reported the mountain as constantly quivering like a boiling pot. At Kapapala in Kau, at the base of the mountain, both foreigners and natives assert that they distinctly heard the swash of the fiery liquid, like the roaring and surging of a rushing river. The sheen of light which rose thousands of feet heavenward, and spread like a burning firmament over the mountain, was truly magnificent. At times the splendor was so vivid and so extended that observers called out the whole neighborhood to witness the scene; some thought they saw the fiery river rushing down the side of the mountain; and numbers were sure that it was half way down the side, and that it was coming toward Hilo in hot haste. This, however, proved an optical delusion. The molten sea was confined within the deep crater, but it was fearfully grand. Parties were planning a visit to the scene of action, when suddenly the great furnace ceased blast. This was a little tantalizing, but as we had all been favored with free tickets to a royal display of fireworks, we could not mourn.

Kilauea has been very active for months, and vast changes have been made in the great pit. The overflowings within it have been frequent and abundant; hills of lava have been heaped up in the southern part of the crater, and the deep central basin is fast filling."

5. *Geology of Ohio.*—The first part of the Final Report on the Geology of Ohio, under the charge of Prof. J. S. Newberry, is just now leaving the press. It constitutes the first half of the first volume, and treats of the Geology of the State, and will extend, as we learn from Dr. Newberry, to 680 pages, and contain 25 maps and sections. Part II. of the same volume treats of the Paleontology, and will make about 450 pages, and be illustrated by 50 plates. This second part is promised by July 1st.

The sheets of nearly the whole of Part I, and some of the finished plates of the Paleontology, are now before us, and they show that Dr. Newberry, and his associates in the work, have placed the State under great obligations to them by their labors. Among the important questions in American geological history apparently settled by the survey is the fact that the "Cincinnati uplift," raising the region from Lake Erie southwestward into Tennessee, took place at the close of the Lower Silurian. We defer a further notice until a complete copy is received.

6. *The Upper Coal Measures west of the Alleghany Mountains;* by J. J. STEVENSON, Prof. Geol. Univ. City of New York. 30 pp. 8vo. (Am. Lyc. Nat. Hist. N. Y., x, 226; also Salem, Mass.)—The author of this memoir shows, from a careful study of the region of western Pennsylvania and Ohio, and its various coal beds and accompanying strata, that the Cincinnati axis had its highest elevation before the deposition of the Upper Coal-measures began; that therefore the Upper Coal-measures of this region and of Indiana and Illinois were never united, and probably not the Lower Coal-measure basins. The Upper Coal-measures originally extended as far west as the Muskingum River in Ohio. The paper contains many sections illustrating the relations of the Pittsburg and higher coal beds.

7. *Report of Progress of the Geological Survey of Canada for* 1871–72; by ALFRED R. SELWYN, F.G.S., Director. 154 pp. 8vo. Montreal, 1872. (Dawson Bros.)—This volume contains a valuable report by Mr. Selwyn on a journey to British Columbia, and an examination of the geology of the region, including the coal beds of Vancouver Island; also a special Report on the Vancouver Coal fields, by Mr. Richardson; on the Plants, by Dr. Dawson; and on the Coal, by Dr. T. Sterry Hunt. There are other reports on the geology of Canada, by Mr. Bell, Mr. McOuat, Mr. Vennor; of New Brunswick, by Mr. Bailey; and mining and mineral statistics, by Mr. Robb. The genera of the plants collected from the Vancouver coal beds (at Naniano and North Saanich) are, according to Dawson, *Tænioptevis*, *Taxodium* (*T. cuneatum* Newb.), *Sequoia* (*S. Langsdorffii* Heer), *Sabal* (a fan-palm), *Palmacites* (fragments of a leaf), *Populus*, *Quercus*, *Platanus*, *Cin-*

namomum (*C. Heeri* Lsqx.), *Taxites*, *Cupressinoxylon.* Dr. Dawson states that the plants led Lesquereux and Heer to refer the beds to the Tertiary, they being nearly allied to the Miocene; but that Newberry has shown that the evidence of the associated marine fossils makes them Cretaceous, which is the opinion now generally accepted, the species including *Ammonites*, *Baculites*, etc.

8. *Transactions of the Edinburgh Geological Society.* Vol. II, Part I.—This number of the Transactions of the Geological Society of Edinburgh contains the proceedings of the Society from November, 1869, to April, 1872, and includes many excellent papers. They range through all departments of geological science, while chiefly occupied with the geology of different parts of Scotland. The number opens with the address of Archibald Geikie, Esq., Director of the Geological Survey of Scotland, and President of the Society, in which some important points in geology are ably discussed. He offers just criticisms on the uncertainties and inconsistencies in lithological science, even that of Germany, where the subject has received most attention. The fact that specimens of *dolerite*, *anamesite*, *basalt*, *amygdaloid*, or *amygdaloidal dolerite* and *tachylite* (obsidian-like) may all be collected from a single dike in Scotland, is mentioned as an example of the multiplying of names and divisions, without sufficient distinctions, and as evidence that the geological characters and relations of the rocks have not been properly considered by those who have drawn out the systems of classification. The evil from this source is great. Rocks cannot be treated and arranged as if chemical or even as mineral compounds, or on the basis of any physical characters, by mere laboratory work, without a loss of three-fourths of all that is of geological interest in their relations.

9. *Annual Report of the State Geologist of New Jersey for the year* 1872. 44 pp. 8vo. Trenton, N. J., 1872.—This Report of Prof. G. H. Cooke is occupied with valuable information respecting the ores and mines, and various economical mineral products of the slate. He mentions the opening of a mine of mica, a mile north of Broadway, in Warren Co., in a granite vein intersecting gneiss. Some of the plates of mica are more than a foot across.

10. *Das Elbthalgebirge in Sachsen*, von Dr. H. B. GEINITZ.—The second number of the second part of Dr. Geinitz's work appeared near the close of 1872. It contains descriptions and figures of the Brachiopoda and Pelecypoda of the Middle and Upper Quader. The figures occupy seven crowded plates.

11. *A Myriapod in the Permian.*—Dr. Geinitz has described and figured (Sitz. Nat. Ges. "Isis," 1872, pp. 125) a Myriapod from the Permian (Rothliegende or Dyas) of the vicinity of Chemnitz. He calls it *Palæojulus dyadicus*, a name that indicates its relations to Iulus.

12. *Tafeln zur Bestimmung der Mineralien*, von FRANZ V. KOBELL. 108 pp. 12mo. München (J. Lindauer).—The *tenth* edition of von Kobell's well known and excellent tables for the determination of minerals has just been issued at Munich. It is an indispensable aid to the student in mineralogy.

13. *Supposed evidence of Man in the Miocene near the Dardanelles.*—J. LUBBOCK communicates to "Nature," of March 27th, the information that a letter from Mr. F. Calvert to Mr. E. Calvert announces the discovery in beds, regarded as Miocene Tertiary, of bones supposed to be of the Mastodon or Dinotherium, having on them etchings of figures of animals.

II. BOTANY.

1. *Nervation of the Coats of Ovules and Seeds.*—A brief article by Van Tieghem in Comptes Rendus, Aug. 14, 1871, and Ann. Sci. Nat., Nov., 1872, and a long one in the latter Journal by LeMonnier (apparently Van Tieghem's pupil), develop clearly the former's view respecting the morphological nature of the ovule. He deduces the foliar nature of its envelope from its "libero-vascular system," which is that of the leaf. It answers, as has been before explained, to a marginal lobe of a carpellary leaf transformed and convolute around the nucleus, which, being destitute of vascular tissue, is a "parenchymatous excrescence," a *trichome*, to use the recent term of the Germans. LeMonnier sums up the conclusions thus: 1. The ovule always consists of a lobe of a carpellary leaf, folded around a cellular *mamelon* inserted upon the medial line of the lobe: 2. in Angiosperms upon the upper or *trachean* face of the leaf; in Gymnosperms upon the lower or *liberian* face. 3. The embryo, although discontinuous from the tissues of the mother plant, has determinate relations of position: not only is the radicular extremity always directed to the micropyle, but its principal plane is generally perpendicular to or parallel with that of the seminal lobe. 4. The primine, characterized by the presence of vascular bundles, is commonly the only membrane which persists in the mature seed; the secundine, except in rare cases (*Euphorbiaceæ*), is only a deduplication of the primine, and is mostly transitory. A. G.

2. *Supposed American Origin of Rubus Idæus.*—Our cultivated Raspberry is an importation from Europe. Our native Red Raspberry, *R. strigosus*, however, is so near it that the specific distinctness has been in doubt; and specimens from British America and the Rocky Mountains certainly occur which a botanist must needs refer to *R. Idæus* itself. In his studies of the European *Rubi*, Prof. Areschoug (in Botaniska Notiser, 1872, and in a translation by himself in Trimen's Journal of Botany, April, 1873, p. 108, etc.) makes prominent and important the fact that *R. Idæus* has no near relative, or in other words, is the sole Raspberry, in Europe, but in mode of growth, in the bark, etc., as well as in the fruit, accords with American species,—with one of them so closely that all who have come to the conclusion that species have a history must needs infer a community of origin. Areschoug concludes, accordingly, that "this species did not originally have its home in Europe, but its origin is to be found in the east of Asia, viz: Japan and the adjacent countries, or perhaps in North America." It is one of the members of that old boreal flora (as we suppose) now mainly East Asiatic and North American, which

has found its way to, or held its place, in the north of Europe somewhat exceptionally. Both *R. strigosus* and *R. Idæus* inhabit Japan and Mandchuria, and Maximowicz regards them as forms of a common species. Prof. Areschoug adopts the now familiar idea "that the Asiatic and North American floras have reciprocally mixed with each other by passing Behring's Straits and the islands which in its neighborhood form a bridge between the two continents;"—which is a partial explanation of a problem that has to be treated far more generally now that we have reason to believe that this flora formerly filled the Arctic zone. He thinks, moreover, that the simple-leaved frutescent species (also extra-European) are the ancestors of those with divided leaves,—but this is a speculation of a different character, upon which little or no evidence can be brought to bear. A. G.

3. *Gelsemium has dimorphous flowers*, the stamens and the style reciprocally long and short. This was observed by Mr. Canby and myself this spring, but the long-stamened condition is the most common. It has already been noticed by Chapman, but it is worth calling attention to, as it was overlooked in Gray's Manual, as well as by A. DeCandolle and Bentham in their monograph of *Loganiaceæ*. The stipules are reduced to minute and glandular, deciduous points. A. G.

4. "*A New Textile Plant*, allied to the Nettles (*Laportea Canadensis*), has recently been imported from the Alleghany Mountains into Germany by M. Roezl. The plant is perennial and capable of enduring the climate of central Germany. Further experiments are needed ere the commercial value of the plant can be determined." *Gardener's Chronicle.*—Some in this country are old enough to remember a former trial. It was taken to England and Ireland fifty years ago by a Mr. Whitlow, with much ado, and was to take the place of flax. A. G.

5. *Hooker's Icones Plantarum.*—Part II, of Vol. II, new series, just issued, contains plates 1126 to 1150. The figures are chiefly of *Rubiaceæ* and *Compositæ*, and illustrate the new part of the *Genera Plantarum*. *Luina hypoleuca*, of Lyall's collection in Oregon (and which has lately been detected in California), is the only North American plant in this fasciculus. The name is evidently an anagram of *Inula*. The genus is probably too near *Tetradymia*, which sometimes has glabrous achenia. From the figures of the Rubiaceous genus *Heterophyllæa*, it may be rather confidently surmised that it has dimorphous flowers, in the manner of *Houstonia*, *Mitchella*, etc. A. G.

6. *Bentham and Hooker, Genera Plantarum.*—Part I. of Vol. II. of this most important work was published in April, and has come to hand. It contains, including an index, 554 pages: the whole, except about a dozen pages, devoted to the two great orders, *Rubiaceæ*, 337 genera, and *Compositæ*, of 766 genera, notwithstanding a very great reduction of old genera. A critical notice of this work must be deferred. The title page states that the Genera Plantarum is sold in London, by Lovell Reeve & Co.,

and by Williams & Norgate. It may be ordered from any principal bookseller. But any botanists who find it difficult to procure the work otherwise, may be supplied upon application to Harvard University Herbarium. A. G.

7. WM. S. SULLIVANT.—This distinguished Bryologist and most admirable man died at his residence, Columbus, Ohio, on the 30th of April last, after an illness of about three months, at the age of 70 years. A biographical notice will be given in the ensuing number of this Journal. A. G.

III. ASTRONOMY.

1. *Telescopic Observations of Meteors.*—Dr. Galle, of Breslau, has recently discussed the interesting question whether multiple meteors enter our atmosphere in flights, or owe their separation into discrete bodies to the effects of explosion. He remarks that several considerations seem to suggest the former theory, and quotes in its favor some telescopic observations recently made on meteoric bodies. Such observations are so seldom effected (simply because a telescope cannot be turned upon shooting stars, and the chances are enormously against the accidental passage of any of these bodies across the telescopic field of view), that great interest attaches to the few that have been recorded, especially when meteors have been seen with telescopes of considerable power. Two observations, both by Dr. Reimann, were recently announced, and the Königsberg heliometer was the instrument with which the observations were made. In the first case, three small meteors, separated from each other by small dark spaces, were seen to travel together across the telescopic field. The two in front were smaller than the third, and the three presented the appearance of a small isosceles triangle, whose base traveled in front—thus, ·: These bodies moved so slowly that they could be conveniently watched. This slow motion implies great distance, yet they were as bright as stars of the fourth magnitude. The observer formed no estimate of their apparent dimensions. The bodies showed no trains. In the second case, a small meteor passed across the field of view, in whose track, at a distance of about a quarter of a degree, followed a fainter meteor.

Dr. Galle remarks that the number of such observations is not large. Most of those made before the year 1860 are collected in a communication from Haidinger, read before the meeting of the Vienna Academy in February, 1861, and relating to the double meteor of Elmira and Long Island. Galle considers that if telescopic observations could be oftener effected, the number of cases of multiple meteors could be largely increased. One of the most striking instances of a multiple meteor was the one observed by Schmidt at Athens, October 18, 1863. In that case, the naked eye could recognize only what appeared to be a single meteor, but in the telescope two large meteors could be seen traveling in front of a number of small fireballs, each of which was followed by a train. The well-known skill and accuracy of Schmidt and the length of time (14 seconds) during which the object continued in the telescopic field, renders this observation peculiarly valuable.

Dr. Galle considers that his researches into the phenomena presented by the meteors which fell at Pultusk on January 30, 1868, as a rain of stones, demonstrate that the meteors were separate long before they reached the place of so-called explosion, and that this place is only the spot where a complete resistance to the planetary velocity and a partial rebound from the impressed air take place, and whence the meteor falls with a velocity corresponding to the law of terrestrial gravity. Haidinger, from certain physical features of fallen meteors, had already inferred the necessity of the theory that the separate meteors had followed distinct paths through the air. Dr. Galle considers that at present it may be regarded as still an open question, whether meteorites enter our atmosphere, from outer space, already separated so as to form a swarm, or whether, shortly after entering and during their passage through the air, they are reduced through the effects of heat into smaller fragments, which the more or less freshly broken appearance of many fragments, as distinguished from the full or partial over-crusting of others, seems to indicate.

He notes as unusual, in the first observation by Dr. Reimann, the circumstance that the two meteors traveling in front were smaller than the one which followed them.—*Monthly Notices, Feb.*

2. *Origin of Meteoroids and Aërolites.*—An interesting speculation by Professor Schiaparelli on the hyperbolic velocities of some recently observed aërolites and fireballs,* occurs in his last published work on the "Astronomical Theory of Meteors," relating to the question of the possible identity, or of the separate origin of these meteors, and of ordinary shooting-stars or meteor-showers. Rejecting, on apparently sufficient grounds, as fallacious, the conclusion of Laplace, that if comets, before entering the sphere of the sun's attraction, are supposed to be traversing space with various velocities in various directions, the probability of their attaining the immediate neighborhood of the sun in parabolic orbits is many thousand times greater than the probability of comets with hyperbolic orbits approaching it so closely as to become visible from the earth; and adopting the exactly opposite conclusion that the frequent occurrence of parabolas, and the entire absence of hyperbolas of any very great excentricity among the orbits of non-recurrent comets indicates them all to be originally journeying in space with nearly the same velocity, and in nearly tho same direction as the sun, Professor Schiaparelli regards these bodies as the original inmates, or portions of one of the "star-drifts," of whose existence very decided proofs have lately

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* Six instances of aërolitic fireballs, of which hyperbolic velocities were credibly computed, are cited by Schiaparelli in his work (translated from the Italian by Dr. von Boguslawski), *Entwurf einer Astronomischen Theorie der Sternschnuppen*, p. 207, *et seq.*

been obtained by Mr. Proctor; and as composing, with other stars of the same vast eddy, attendant bodies accompanying in its journey through space the general "drift" or star-family, of which the sun itself forms a part. On this assumption, aërolites and meteors moving with hyperbolic velocities are bodies from more distant spaces than the star-family of the sun, or wanderers from the regions of more distant star-drifts, whence they have, possibly, been projected, with sufficient initial velocities to escape from their spheres of attraction by the stars themselves; and their origin is, in this case, entirely different from that of comets, and of meteoric showers. If, as Professor Schiaparelli observes, this be the real explanation of the high velocities occasionally met with among the best recorded descriptions of aërolites and fire-balls, the evidence already obtained by the spectroscope of a general unity of composition among the remotest fixed stars is even more remarkably extended by the analysis of meteorites to the utmost limits of the starry sphere. But if the innumerable crowd and weight of the stellar fragments which this hypothesis supposes should appear to offer an insurmountable objection to its reception, the only obvious alternative remaining open to conjecture is to regard the occasionally observed high velocities of aërolites, or fire-balls, as constituting very rare exceptions; and the generality of both aërolites and shooting-stars to be moving in orbits, like the comets, with velocities which seldom greatly surpass the speed communicated to them by the sun's attraction, and as falling toward it from spaces not more distant than those of the parent eddy, or "star-stream," whose drift, or motion of translation in space, is found to be in general nearly similar to the proper motion of the sun.—*Ibid.*

IV. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *The National Academy of Sciences* held its annual session at the Smithsonian Institution in Washington, D. C., April 15th–18th, 1873, when the following papers were presented:

The determination of singular points of Curves and Surfaces by the method of quarternions; Benjamin Peirce.

The geodesy of the Coast Survey; B. Peirce.

Silt analysis of Soils and Clays; E. W. Hilgard.

On the meteoric iron found in 1871, near Shingle Springs, Eldorado County, California; B. Silliman.

Experimental and graphic results of distilling certain hydrocarbons by heat, with and without the aid of vacuum and steam; C. F. Chandler and B. Silliman.

On the structure and age of the Cincinnati anticlinal; J. S. Newberry.

On the need of more accurate investigations and tables of the celestial motions; Simon Newcomb.

On the general Atmospheric circulation; A. J. Wocikof.

Determination of longitude between Europe and America, by the U. S. Coast Survey; J. E. Hilgard.

On some peculiarities in two recently discovered minor Planets; J. C. Watson.

On the altitudes of Gray's and Torrey's peaks in Colorado Territory, and some questions connected with the determination of barometric altitudes in the interior of continents; A. Guyot.

On the unity of the system of life in animals and the true principle of gradation in the various animal types; A. Guyot.

On repeating curves; H. A. Newton.

On the stability of the Meridian Circle of the Observatory of Harvard College; Joseph Winlock.

On some experiments made with a slitless Spectroscope in 1871, in order to see the whole chromosphere of the sun at once; J. Winlock.

On observations of the Sun made at the Observatory of Harvard College in 1872, with the aid of the Bache Fund; J. Winlock.

On a method of illuminating the threads of the reticule of a telescope by the electric spark; J. Winlock.

Comparison of the Spectra of the limb and of the centre of the Sun, made at the Sheffield Scientific School; Chas. S. Hastings, read by H. A. Newton.

On certain harmonies of the Solar System; Stephen Alexander.

Report of progress of a Magnetic Survey made by the aid of the Bache Fund; J. E. Hilgard.

Eulogies were also read on deceased members of the Academy: on Dr. John Torrey, by Prof. Asa Gray; on Prof. William Chauvenet, by Prof. J. H. C. Coffin.

The following members of this Academy have died during the year 1872–73: J. H. Coffin of Easton, Pa.; James Hadley of New Haven; John T. Frazer of Philadelphia; William Stimpson of Chicago; and John Torrey of New York.

The following new members were elected: Theodore Gill of Washington, D. C.; Elias Loomis of New Haven; Joseph Lovering of Cambridge, Mass.; William A. Norton of New Haven, Conn.; and J. J. Woodward of Washington, D. C.

2. *Reports of Explorations and Surveys to ascertain the practicability of a ship canal between the Atlantic and the Pacific oceans by the way of the Isthmus of Tehuantepec;* by R. W. Shufeldt, Capt. U. S. N. 151 pp. 4to, with 20 maps. (Made under the direction of the Secretary of the Navy.) Washington, 1872.—The Report of Capt Shufeldt contains a particular account of the region surveyed, with some lithographic views. It is followed by tables of heights and distances; of Building Materials on the routes, giving the occurring rocks; of Useful Trees, Plants, &c.; of Languages of the Aboriginal Tribes, and a Report of the Hydrographic Surveys on the Atlantic Coast, by Lieut.-Comm. N. H. Farquhar, U. S. N., and on the Pacific Coast, by Lieut.-Comm. A. Hopkins, U. S. N. Map No. 18 is a colored geological chart of the isthmus of Tehuantepec, showing the limits of the different rocks, but without indicating the period to which they belong.

3. *Manual of Chemical Analysis, as applied to the Examination of Medicinal Chemicals:* a guide for the determination of their identity and quality, and for the detection of impurities and adulterations. For the use of pharmaceutists, physicians, druggists, and manufacturing chemists, and of pharmaceutical and medical students. By Frederick Hoffman, Ph.D. 393 pp. 8vo, with 96 wood-cuts. 1873. New York (D. Appleton & Co.)—This title fully explains the object of Dr. Hoffman's Manual, which is a carefully prepared book, and well up to the existing state of both the science and art of modern Pharmacy. It is a book which will find its place in every medical and pharmaceutical laboratory, and is a safe and instructive guide to medical students and practitioners of medicine.

APPENDIX.

ART. LIII.—*Notice of New Tertiary Mammals* (continued); by O. C. MARSH.

THE present paper is a continuation of that on page 407. The remains here described were nearly all collected by the late expeditions from Yale College, and the type specimens are preserved in the Museum of that institution.

Tillotherium hyracoides, gen. et sp. nov.

This genus presents some remarkable characters in its dentition, which separate it widely from any described, with the possible exception of *Anchippodus* Leidy, to which it may prove to be nearly related, when additional remains of that genus are discovered. There were two large incisors in each premaxillary, the inner and larger one being gliriform, and covered on its front and outer faces with enamel. The canine was small and directed well forward. There appear to have been four upper premolars, increasing in size posteriorly. There are three true molars, the last being the largest. They are all much greater in transverse than in antero-posterior extent, and this is especially true of the last. They are composed essentially of a pair of external cones, connected with a single internal lobe by two oblique converging ridges. There is a small tubercle in the depression thus enclosed. The basal ridge on the posterior side is expanded, forming a low shelf. The antero-external cone has an outer cusp, which projects outward and forward. The present species was about two-thirds the size of a Tapir. The large upper incisors are sub-triangular in transverse outline, the posterior face being concave. The lower jaws and skeleton are not known with certainty. It is possible that the present remains may prove to be generically identical with *Anchippodus minor* Marsh (*Trogosus castoridens* Leidy).

Measurements.

Antero-posterior diameter of large upper incisor,	21· mm·
Transverse diameter,	15·
Space occupied by last three upper molars,	59·
Antero-posterior diameter of first upper true molar,	16·5
Transverse diameter,	29·
Antero-posterior diameter of last upper molar,	21·
Transverse diameter,	38·

The known remains of this species are all from the Eocene of Wyoming.

Brontotherium gigas, gen. et sp. nov.

An examination of the remains, in the Yale Museum, of the huge mammals allied to *Titanotherium*, has led to the discovery that two different animals have hitherto been referred to the species known as *T. Prouti*. These animals are generically distinct, and probably are from separate geological horizons. The one here described differs from *Titanotherium* in its dentition, having but three lower premolars, the series being as follows:—Incisors 2, canine 1, premolars 3, molars 3. The animal was, moreover, a true Perissodactyl, with limb-bones resembling those of *Rhinoceros*. The genus is related to *Titanotherium** and the two appear to form a distinct family, which may be called *Brontotheridæ*.

The present species is based on portions of three individuals, one of which has the lower jaws and entire molar series complete. They indicate an animal fully equal to *T. Prouti* in size, and but little inferior in bulk to the Elephant. The lower molars resemble those in the type specimen of *T. Prouti*, but the jaw below them is not so deep, and its lower margin is more nearly straight, descending but very slightly toward the angle. The front part of the lower jaws is somewhat suilline in form. The incisors are quite small, and the two next to the symphysis are separated from each other. There is a short diastema between the canine and first premolar.

From the other specimens preserved, the greater part of the skeleton can be made out. It closely resembles that in recent Perissodactyls, but shows some approach to the Proboscidea. The femur has a third trochanter, and its head a pit for the round ligament. The fibula is entire, and slender. The astragalus is remarkably short. It has a deep groove on its upper surface, and the articular facets for the navicular and cuboid are nearly equal. In the manus there are four toes of nearly equal size, the first digit being rudimentary or wanting. There were three digits only in the pes, the first and fifth being entirely wanting. The toes were short and thick, as in Proboscidians. The metacarpals and metatarsals are longer than in the elephant, and the phalanges shorter. The foot was also more inclined. The carpal and tarsal bones are very short, and form interlocking series. The tail was long and slender.

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* The generic name *Titanotherium* Leidy is antedated by *Menodus* Pomel (Bib. Univ. de Genève, x, p. 75, Jan., 1849). The latter, however, is essentially the same word as *Menodon* von Meyer, 1838, and is also objectionable in its form; hence *Titanotherium* should be retained.

Measurements.

Length of lower jaw, from condyle to front of symphysis,	634· mm.
Depth of lower jaw, from top of coronoid process to angle,	367·
Depth below front of last lower molar,	123·
Depth below first lower molar,	113·
Length of symphysis,	122·
Length of last lower molar,	117·
Length of last lower premolar,	51·
Transverse diameter,	35·
Length of first lower premolar,	31·
Transverse diameter,	21·
Distance between lower canines,	28·5

The remains on which the above description is based were found in the Miocene of Colorado by Mr. H. B. Sargent, Mr. J. W. Griswold, and the writer.

Elotherium crassum, sp. nov.

A large suilline mammal, which probably belongs in the genus *Elotherium*, is indicated by portions of two skeletons, in the Yale Museum. These specimens present some features not before observed in any Ungulates. The most striking of these is a very long process descending from the malar bone, and giving attachment to the masseter muscle. This process resembles somewhat the downward prolongation from the zygomatic arch in some Edentates and Marsupials, but it is longer, and more compressed. The radius and ulna were separate, or very loosely united. The third and fourth metacarpals were nearly equal in size, and the second and fifth larger than the corresponding bones of the pes. In the latter the first digit was wanting, and the fifth rudimentary. The hoof phalanges were short. The tail was long, and quite slender. This species is intermediate in size between *E. Mortoni* and *E. ingens.*

Measurements.

Length of malar process below squamosal suture,	130· mm.
Length of symphysis of lower jaws,	144·
Antero-posterior diameter of lower canine,	32·5
Transverse diameter,	28·5
Transverse diameter of humerus at distal end,	81·
Transverse diameter of radius at distal end,	75·
Transverse diameter of head of tibia,	81·
Length of third metatarsal,	102·

A rather smaller specimen, apparently of the same species, afforded the following

Measurements.

Length of symphysis,	122·mm
Depth of jaw below first premolar,	57·
Depth below last lower molar,	69·
Space occupied by lower molar series,	216·
Space occupied by three lower true molars,	76·

All the known remains of the present species are from the Miocene of Colorado.

Yale College, May 5th, 1873.

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